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General Astronomy.

METEORIC ASTRONOMY.

I.

THE LEONIDS, OR METEORS OF NOVEMBER 13.*

DANIEL KIRKWOOD.†

Within the memory of persons now living shooting stars were regarded as gaseous matter generated in the atmosphere. Their true nature was wholly unknown, and works on astronomy made no attempt to account for their origin.

The most brilliant display of these phenomena recorded in history occurred on November 13, 1833. Few persons of the present day remember the scene as then witnessed. A shower of fire, indeed, is not to be forgotten, but the interval of 60 years has left the number of spectators small. Throughout a large part of North America the atmosphere on the night of the display was remarkably clear. The unusual frequency of meteors was noticed as early as eleven o'clock on the night of the 12th. As the night advanced their numbers rapidly increased, till all attempts to count them were entirely abandoned. Thousands of observers, from Greenland to Florida—from Behring's Strait to Panama, looked on with increasing wonder from midnight to daylight. The meteors appeared to radiate from a particular point in the constellation Leo. Their apparent magnitudes varied from the smallest visible particles to that of the full moon. Their brilliancy was such that persons sleeping in rooms with uncurtained windows were aroused by their light. Occasionally one of the larger masses separated into parts, and some fragments are said to have remained visible for several minutes. In some parts of the country the ignorant and superstitious were completely terrified, imagining that the last day was about to break upon the world. The wonderful appearance presented a new problem for

* Communicated by the author.

† As the closing years of the nineteenth century are to witness a display of the November meteors, the following paper may have a timely interest.

scientific investigation. Shooting stars had indeed been seen in all ages of the world, but they had been looked upon as simply atmospheric phenomena. It was now seen, however, that the meteors of November 13 did not move in harmony with the ancient theory. A new discussion of facts was accordingly undertaken by prominent astronomers. Among its leaders were Professors Olmsted and Twining of Yale College, Olbers and Erman in Germany, with others no less eminent both in Europe and America. The origin of Meteoric Astronomy, as a science, dates from this epoch. It was remembered by a few persons that at the same date in the previous year, 1832, an unusual number of meteors were seen, though the display was much less brilliant. It was perceived, therefore, that this coincidence of dates could be best explained by supposing a train of cometary matter to cross the Earth's path at the point passed by it on the 13th of November. The fact was also recalled that Humboldt had witnessed a similar display though less remarkable, in South America, November 12, 1799; thus confirming the theory of a cometary or nebulous intersection of the Earth's orbit, and indicating a period of 33 or 34 years. The *American Journal of Science* for 1864 contains the elaborate researches of Professor H. A. Newton in regard to former displays of the November meteors. The list extends backward through a thousand years, including the dates 1698, 1602, 1533, 1366, 1202, 1101, 1002, 934, 931 and 902. The shower of 1366—almost equal to that of 1833—is thus described in a Portuguese chronicle quoted by Humboldt:—

"In the year 1366, twenty-two days of the month of October being past, three months before the death of the king, Don Pedro (of Portugal), there was in the heavens a movement of stars such as men never before saw or heard of. At midnight, and for some time after, all the stars moved from the east to the west; and after being collected together, they began to move, some in one direction and others in another. And afterward they fell from the sky in such numbers and so thickly together, that as they descended low in the air they seemed large and fiery, aud the sky and the air seemed to be in flames, and even the Earth appeared as if ready to take fire. That portion of the sky where there were no stars seemed to be divided into many parts, and this lasted for a long time."

With the average time of revolution (33¼ years), the perihelion being at the Earth's orbit, it is easy to calculate the meteoric path around the Sun, as represented in Fig. 1.

The return of the November meteors have been either after inter-

vals of 33 years, or after some multiple of that period. A return in 1866 or 1867 was accordingly predicted. The shower was witnessed in Europe in the former year and in America in the latter. The fact that the phenomena extend over two or three years merely indicates the gradual diffusion of the meteors along the orbit, so that several years are required for the entire cluster to pass any particular point of the path.

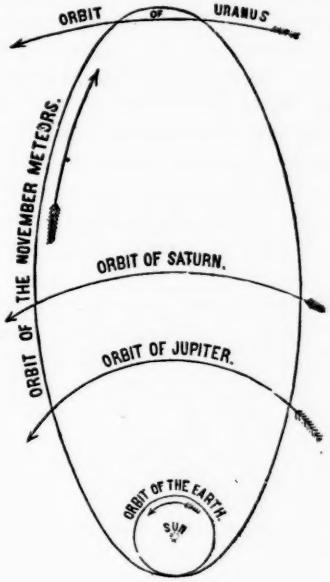


FIG. 1.

Origin of the Meteors of November 13th. In December, 1865, less than a year before the great showers of the November meteors, a comet was discovered moving in the same path, in the same direction, and having the same period. This coincidence could not have been accidental. It was accordingly concluded that the meteoric cluster was derived from the comet; that the latter is undergoing the process of dissolution; that the cometary matter is being slowly distributed around the orbit; that the comet will thus finally disappear and manifest itself only as yearly showers of the November stream.

Height of Meteors. The observations of Professors Newcomb, Harkness and Eastman, of the U. S. Navy, on the 12th, 13th and 14th of November, 1867, decided, at least approximately, several questions of importance. A line from Washington, D. C., to Richmond, Va., furnished a convenient base for the determination of parallax. It was found that the meteors first became visible at an average altitude of 75 miles; the maximum probably not much exceeding 100 miles. The average height at extinction was about 55 miles; the latter elevation being greater than the previously estimated height of the atmosphere. As the meteors of this swarm have retrograde motion, or, in other words, meet the earth in its path, they have the extraordinary relative velocity of 44 miles per second. This rapid motion, even in an atmosphere extemely rare, produces an intense heat of

the moving particles. The brilliancy of many was very remarkable. In a majority of instances visibility lasted but a fraction of a second.

During the thickest part of the shower (1867) the meteors were counted at the rate of 50 per minute, or one nearly every second. These small bodies are burned up or dissipated long before reaching the Earth's surface. The results of combustion, however, may, at least in part, be ultimately deposited as solid matter. The weight of the individual meteoroids cannot be given with any degree of accuracy. They are known, however, to be very minute, and Professor Newcomb remarks that if we assume a mean weight of a single grain "the entire mass of the stream may be rudely estimated as equal to that of a mass of iron 400 feet in diameter."

Such has been the development of meteoric astronomy. A *pseudo* science of atmospheric phenomena, it has, within three quarters of a century been transferred to the heavens. From tracing the "mystic dance" of *ignes fatui*, it has risen to trace the curves of comets and of stars. In determining the cometary period, Professors H. A. Newton, of Yale College, and J. C. Adams, of Cambridge, England, held each a conspicuous place. The former showed the period to be 180.05 days, 185.54 days, 354.62 days, 376.5 days, or 33.25 years. He showed, more over, how it was possible to determine which of the five periods is the correct one. This crucial test was applied by Dr. J. C. Adams, who, by an elegant analysis, proved the last named period to the true one. * Professor Newton has traced the history of the Leonids for a thousand years—from A. D. 900 to the present time. Its ancient track is more obscure, but an occasional glimpse may perhaps be caught as it moves down successive ages. Were the cometic matter uniformly distributed around the orbit a display would be an annual occurrence. A few sporadic outriders of the cluster are sometimes found, and rarely, larger groups have been noticed. Two have been specified by by the present writer. † *When may we expect the next shower from this celebrated cluster?* The average period, as we have seen, is 33.25 years, and the display is continued through at least three consecutive years. We may therefore expect it in 1899, 1900, and 1901. In former times it not infrequently escaped observation. The meteoric fall sometimes occurred in the daytime, sometimes in cloudy weather, and sometimes over the

* *Monthly Notices* R. A. S. for April, 1867.
† See Payne's Sid. Mess., Oct. 1885. Also Chambers' Astr., Vol. I, p. 632.

ocean, or over uncivilized countries. The probability of such failures is now less than in former ages. In short, if we live and retain our sight for the next six or seven years we shall no doubt have the pleasure of seeing at least one grand meteoric shower.

It will also be highly interesting to astronomers to watch the comet of 1866, the body associated with these November meteors—the comet moving with them, and from which they are derived. As it has for centuries been subject to a wasting process it was doubtless larger in ancient times than at present. In 1366 and at some other returns before the invention of the telescope, it seems to have been seen by the naked eye. The meteors were less widely spread, and, of course less frequently seen.

THE METEOR COMET OF 1866.

Tempel's comet of 1866 was not visible to the naked eye. It is to be remarked, however, that the apparent magnitude at any return must depend on its relative position with respect to the earth, and also that for a thousand years or more it has been undergoing the process of gradual dissolution. It was a naked-eye comet in 1366 and 868. Other dates also correspond with remarkable exactness to those at which the maximum cluster passed perihelion. Oppolzer's period is 33.176 years, and Newton's for the associated meteors is 33.25 years. The striking agreement of dates at which the maximum cluster passed the perihelion is thus represented:

From	1366	to	1866	15	periods of	33.28 years.
"	1266.8	to	1366.8	3	"	33.33 "
"	1133.8	to	1266.8	4	"	33.25 "
"	1067.4	to	1133.8	2	"	33.20 "
"	1000.9	to	1067.4	2	"	33.25 "
"	868.1	to	1000.9	4	"	33.20 "
"	834.8	to	868.1	1	"	33.30 "
"	668.4	to	834.8	5	"	33.28 "
"	602	to	668.4	2	"	33.20 "
"	436	to	602	5	"	33.20 "
"	336	to	436	3	"	33.30 "
"	269.7	to	336.1	2	"	33.20 "
"	69.7 (?)	to	269.7	6	"	33.30 "
B. C.	295 (?)	to	69.7	11	"	33.20 "

The mean length of the 65 periods, included in the foregoing list is 33.25 years—Newton's period of the meteors; or, if we reject the comet of 295 B. C. as doubtful, we have 54 periods of 33.26 years.

The phenomena of the Leonids fix the dates at which the maximum cluster intersects the earth's orbit, and hence the time of perihelion passage may be inferred. They indicate also what

parts of the ring are destitute of meteoroids; what parts are thinly, and what more densely filled with meteoric matter. A separate group of Leonids has thus been found preceding the main cluster 12 or 13 years.* An ancient display from this secondary cluster is given by Quetelet *Sur La Physique du Globe, p. 290.* The date was A. D. 288, Sept. 28. A phenomenon from this cluster is also given by Hind for 855 and again in 856. Another, 1787, is given in Humbolt's Cosmos, Vol IV. Later sparce showers are mentioned by Chambers. "It is thus that highly important consequences may be expected to be traced from these and similar investigations and discussions; indeed, the subject may perhaps fairly be deemed an inexhaustible one, for a few coincidences having been ascertained, more will be sure to follow as observations multiply and research extends."†

JUPITER'S SATELLITES.‡

WILLIAM H. PICKERING.

Although we are now in the midst of our rainy season, I have been able to continue my observations upon the satellite system at intervals, and have acquired some further information. The satellites taken as a whole have developed so many unexpected phenomena, and what have appeared to be the most natural suppositions have been found so frequently to be contradicted by the facts, that it seemed best to take nothing for granted with regard to them. For this reason it appeared most desirable to determine if possible whether their rotation upon their axes was direct or retrograde. The detail exhibited by their surfaces, excepting in the case of the 3rd, is so difficult that little reliance can be placed upon observations of it, and for the other satellites at least, this method must be discarded.

At first sight it does not seem possible to determine the direction of rotation of a body by simply watching the alternate lengthening and shortening of its disc. Nevertheless, a little consideration will show that it is quite possible theoretically, and the practical solution of the problem depends merely upon the accuracy of our observations. The phenomenon which comes to our assistance at this juncture is the revolution of the Earth in its orbit, for it is quite evident that after the opposition of the planet, if the direc-

* Chambers' Descriptive Astr., Vol. I., pp. 631-2.
† Chambers.
‡ Communicated by the author.

tion of the rotation of the satellite is direct, it will present a given phase rather earlier than it would do if our observations had been made from the center of the Sun. On the other hand is the direction of rotation is retrograde, the presentation of the phase will be somewhat delayed. With the solution of this question in mind I have made a preliminary reduction of our observations upon the 1st satellite, of which I will give a brief synopsis in what follows. The observations are made in Arequipa meantime, the longitude of Arequipa being approximately $4^h\ 45^m\ 30^s$ from Greenwich.

Below is given as a sample the last series of observations made. It was found that the impression of the shape of the disc as seen by the eye gave a much more accurate determination of the period, than could be obtained by a series of micrometric measurements of its length. This will be readily understood when it is considered that the total variation in length in the course of three hours amounts to little more than 0″.1, while a difference between the two diameters amounting to only 0″.01 can be detected by a direct comparison, as is shown by the observations:

Saturday, January 28, 1893.

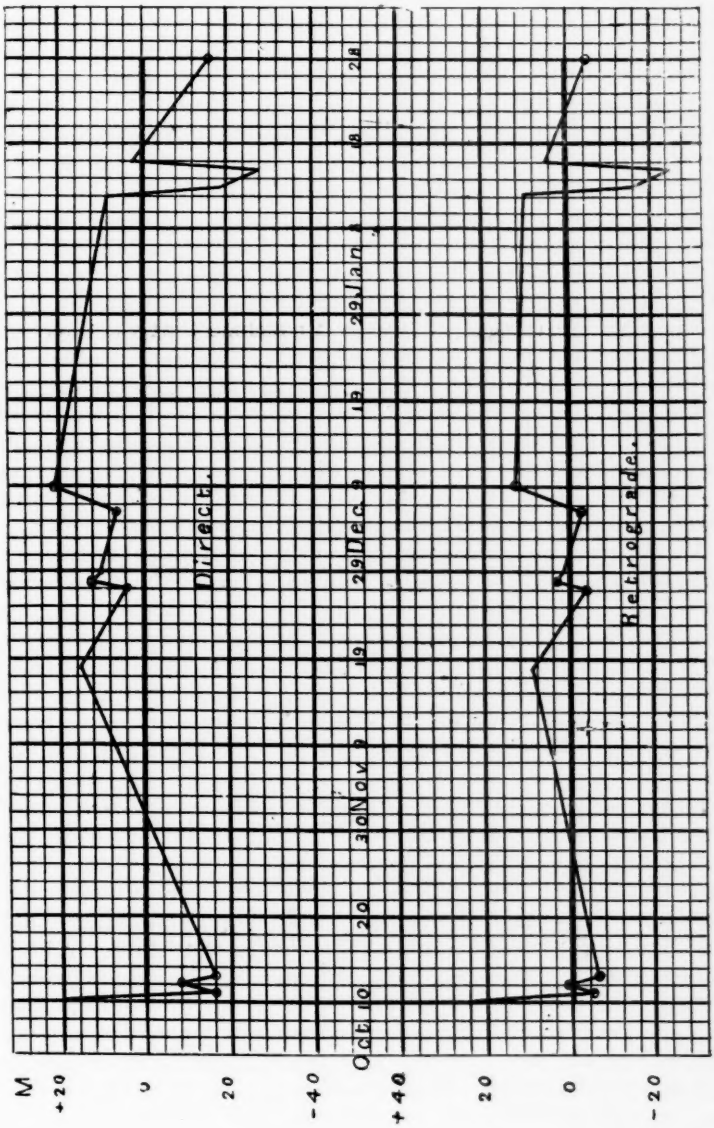
$7^h\ 03^m$ (Watch) 1st satellite long (*i. e.* disc elliptical). Seeing 3 to 4 (5 perfect).
7 36 1st getting rounder, almost orange colored.
7 45 1st still a trifle longer than Jupiter (*i. e.* in proportion to its size).
7 58 1st getting shorter.
8 03 1st still long.
8 06 1st long.
8 10 1st not quite as long as Jupiter probably.
8 15 1st almost round.
8 21 1st no shorter.
8 26 1st nearly round. Seeing 4.
8 33 1st circular.
8 38 1st perfectly circular. Seeing 4.
8 40 1st perfectly circular. Seeing 4.5.
8 45 1st almost elliptical. Haze thicker.
8 50 1st I think getting elliptical. Hazy.
8 55 1st elliptical, but very hazy.
8 59 1st hazy but very clearly elliptical. Circular at 8:40 by watch, or 8:39 by M. T. clock.

From the above it will be seen that to obtain a satisfactory series of observations requires about two hours, and that consequently at the present time, unless the circular phase occurs very early in the evening no complete series can be made. From the middle of October to the middle of November Mr. Douglass and I were absent from the observatory. On my return I had hoped to find the photographic routine work completed, but as it was not, I had to do it myself, which occupied another month. Thus it has been possible to obtain but comparatively few series of observations of this phenomenon this year. These however are given in the following table. Sixteen series were obtained in all, of

which eight are considered satisfactory, and are weighted 2. Seven are considered unsatisfactory, usually because the observations either before or after the circular phase were incomplete. One series was rejected since but two observations were made within forty minutes of the proper time, and they led to a manifestly erroneous result, the circular phase having been supposed to occur after them, when it had really occurred at some time between them.

In the following table the first column gives the current number, the second the observed Arequipa Mean Time of the circular phase, the third the weight attached to the observation, and the fourth the number of rotations that had occurred since the first accurate observation, upon October 11. The next three columns are based on an assumed direct rotation of the satellite, the first being the observed time reduced to the center of the Sun, the second the theoretical time assuming a uniform rotation, and the third column the difference between the two. The last three columns give the same results based on a retrograde rotation. The fourth determination is obtained from a series of observations made by Mr. Douglass. Upon the hypothesis of a direct rotation, the synodic period of the satellite is $13^h\ 03^m\ 25^s.8$, upon the hypothesis of a retrograde motion it is $13^h\ 03^m\ 10^s.8$:

				Direct.			Retrograde.		
No.	Date.	Wt.	R.	Obs.	Theory.	D.	Obs.	Theory.	D.
				d h m	d h m		d h m	d h m	
1	Oct. 10 11 51	1	− 2	10 11 58	10 11 39	+ 19	10 12 00	10 11 37	+ 23
2	11 13 29	2	0	11 13 36	11 13 52	− 16	11 13 38	11 13 43	− 5
3	12 09 11	2	1.5	12 09 19	12 09 27	− 8	12 09 19	12 09 18	+ 1
4	13 11 10	2	3.5	13 11 18	13 11 36	− 16	13 11 18	13 11 24	− 6
5	15 09 52	0	7	15 10 01	15 09 16	(+ 45)	15 09 59	15 09 05	(+ 54)
6	Nov. 18 09 13	1	69.5	18 09 35	18 09 20	+ 15	18 09 03	18 08 34	+ 9
7	27 08 27	2	86	27 08 51	27 08 47	+ 4	27 08 13	27 08 17	− 4
8	28 10 41	2	88	28 11 06	28 10 54	+ 12	28 10 26	28 10 23	+ 3
9	29 06 14	1	89.5	29 06 39	29 06 29	+ 10	29 05 59	29 05 58	+ 1
10	Dec. 6 07 54	2	102.3	6 08 20	6 08 14	+ 6	6 07 36	6 07 39	− 3
11	9 07 57	2	108	9 08 23	9 08 02	+ 21	9 07 39	9 07 26	+ 13
12	Jan. 12 07 50	1	170.5	12 08 15	12 08 07	+ 8	12 07 25	12 07 15	+ 10
13	13 09 31	1	172.5	13 09 55	14 10 16	− 19	13 09 05	13 09 21	− 16
14	15 07 04	1	176	15 07 28	15 07 56	− 28	15 06 38	15 07 03	− 25
15	16 09 40	1	178	16 10 04	16 10 02	+ 2	16 09 14	16 09 09	+ 5
16	28 08 40	2	200	28 09 02	28 09 18	− 16	28 08 14	28 08 19	− 5


Direct.
Retrograde.
Oct 10
20
30 Nov 9
19
29 Dec 9
19
29 Jan 1
18
28
M
+20
0
20
-40
+40
20
0
-20

The average deviation of an observation on the direct hypothesis is $13^m.3$, on the retrograde hypothesis it is $8^m.6$. If we include only the complete observations, which are weighted 2 in the table, we find the probable error of a single observation, if the rotation is direct, is $9^m.0$, if it is retrograde $4^m.0$. An inspection of the differences under the direct and retrograde columns seems also to indicate a systematic deviation in the former case. To make the matter clearer, however, two curves have been constructed, using the dates as abscissæ, and the numbers in the two difference columns as ordinates. If the observations were without error they would lie along a straight horizontal line. The complete observations are indicated by small circles.

As a result of these observations, I conclude that the direction of rotation of the 1st satellite is probable retrograde,—another unexpected result. The only well-known analogy in the solar system being the retrograde revolution of the satellite of Neptune in its orbit, while following the direct revolution of Neptune around the Sun. Further observations before the next oppositions should confirm the retrograde rotation of the satellite by deciding between the two periods proposed.

An explanation of the rotation of the outer satellites about their major axes has recently been found, which is apparently consonant with well-known physical laws, but as the computations necessary to a full development of the theory have not at yet been completed, it is thought best to defer a statement of is until a future paper.

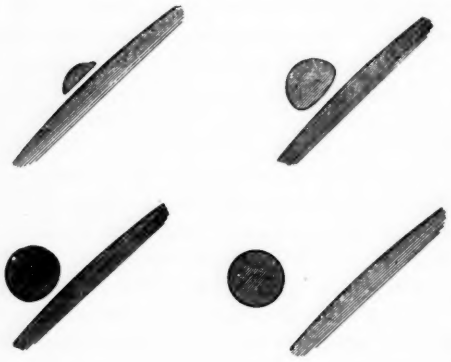
There is so much that is still uncertain in regard to this system that I have thought best to put on record the following observations. I do not, however, wish to express an opinion as to whether they represent actual physical phenomena, or whether they were merely optical illusions. Upon January 13 the 1st satellite appeared distinctly shortened equatorially when at its minimum phase. The appearance lasted for thirty-four minutes, and when first noted the seeing was fairly good, being marked 3.5, but later became worse. This phenomena has never been noted before nor since, although upon one night it was thought that a slight polar flattening had been detected. In our earlier observations the 2d satellite not infrequently appeared long like the 1st. In all of our more recent observations it has appeared round at the maximum phase, like the two outer satellites. At some of its minima the 3d satellite retains the elliptical phase for a longer time than at others. Thus it remained short for three days in succession upon January 13, 14, and 15. Usually this

phase does not last more than one day or two at the most, depending upon the hour at what the elliptical phase takes place.

The observation of December 11, described in my first paper, has been confirmed, and shown to indicate a genuine phenomenon. In that observation the position angle of the elongation of the disc of the 2d satellite was shown to suddenly shift through thirty degrees just before contact with the limb of the planet, prior to occultation, the satellite taking a position parallel to the limb. This change was ascribed to the action of a Jovian atmosphere. As this change was quite unexpected when it was observed it seemed desirable to repeat the observation if possible at the first favorable opportunity. Such an opportunity occurred upon January 12, when the satellite emerged from behind the dark limb of the planet. When first seen the satellite's disc was already half uncovered, and the limb of Jupiter was indented by a deep notch at that point, presumably due to contrast with the bright disc of the satellite. As soon as the emersion was complete, it was seen that the satellite was flattened parallel to the limb of the planet. A few minutes later the position angle of the flattening shifted to its usual position, perpendicular to its orbit, just as occurred in the former observation. Upon January 1 the 1st satellite was watched as it approached the limb previous to transiting. No flattening whatever was noted, however, nor did any change of shape occur at the moment of contact. A similar observation was made upon January 26, when it emerged from the face of the planet. These observations indicate that the change noted in the 2d satellite is not an optical illusion, and that the flattening and change of position angle occur only when the satellite is beyond the planet.

The reappearance of the 3d satellite upon January 27 from behind the dark limb was a more interesting observation, and must be described at length. The first contact occurred at $6^h\ 06^m$ A. M. T., but the second could not be observed advantageously. The position angle of disappearance was almost exactly forty-five degrees from Jupiter's equator. At $8^h 31^m$ I began looking for the reappearance, and at the end of a minute and a half a faint hazy light was seen projecting beyond the terminator of Jupiter. This rapidly brightened, and in one minute more the edge of the limb of the satellite made its appearance, a dark narrow band separating it from the terminator. This band was estimated to be 0″.2 in breadth, and subsequent computation showed that its breadth must have been 0″.16 if there was no appreciable twilight caused by Jupiter's atmosphere. The successive appearances

observed are indicated in the drawings, which are constructed on a scale of one centimeter to the second of arc. As a power of 700 diameters was employed in the observation the proper effect will be obtained if these drawings are viewed from a distance of ten feet. The rounded body represents the satellite, and the long one a portion of the terminator of Jupiter. The diameter of Jupiter would be about fourteen inches. Since the interval between the third and fourth contacts was of ten minutes' duration there was ample time to observe the dark band and estimate its breadth, comparing it with the known breadth of the micrometer threads. By $8^h\ 41^m$ the satellite was more than half uncovered, and it was noted that the cusps were distinctly rounded, as is the case with the Sun, when near the horizon, as seen from a high mountain peak. No notch whatever was seen in the terminator as had been the case with the 2d satellite.



This was undoubtedly due to the less intrinsic brilliancy of the 3d. From its distorted shape it was impossible to tell exactly when the fourth contact occurred. At $8^h\ 46^m.5$ it was noted that the dark band had broadened to $0''.5$ in breadth, but at $8^h\ 48^m$ the satellite was recorded as still flattened slightly. Two minutes later it was perfectly round. At $8^h\ 51^m.5$ it was separated from the terminator by its own diameter and it was recorded that the satellite was but little brighter than the terminator. The interval between the first and the third contacts was $2^h\ 27^m.5$. The geocentric duration of the occultation is given by the almanac as $2^h\ 29^m$.

The first conclusion to be drawn from these observations is that Jupiter is not self luminous, but is only visible when it is illuminated by sunlight. The second conclusion is that it is surrounded by a rare atmosphere outside of its cloud surface, which is capable of producing a measurable refraction. This refraction has been computed, employing the observations at first and third contact, and these when the satellite was separ-

ated from the terminator by 0″.5 and also by its own diameter. Employing the third of these observations as our standard of comparison, the refraction of Jupiter's atmosphere at its cloud surface amounts to 0″.59. Employing the fourth observation as our standard the refraction appears to be 0″.38. The third observation was probably the more accurate, but was partially vitiated since the satellite was not yet free of the planet's atmosphere, which is still sufficiently dense to produce an appreciable effect at an altitude of 0″.8 or 1900 miles above the planet's limb. If we take the atmospheric refraction at the cloud surface, at 0″.50 ± 0″.05 we shall probably be not far from the truth. That the atmosphere should rise to such a great height above the planet's surface was perhaps to be expected from the gradual character of the absorption of the planet's light near the limb. That such a height should be reached in spite of the high gravitation constant in those regions is an independent indication of a high temperature at the planet's surface, and a comparatively low temperature at an altitude of 1900 miles above it. The faint glow seen beyond the dark limb of the planet for about a minute before the satellite made its appearance was doubtless analogous to the same phenomenen seen preceding the rising of our own moon, and may have been caused also in part by the illumination of clouds in the planet's atmosphere too small to be separately visible.

AREQUIPA, Peru, February 18, 1893.

THE PERIOD OF Σ 1785. *

S. W. BURNHAM.

In a paper on the binary star, Σ 1785, printed in *Monthly Notices* R. A. S. for December, 1892, I have given a complete list of the micrometrical measures of this pair from the first observations of South in 1823 to the last measures in 1892, made with the 36-inch at Mt. Hamilton; and also a diagram drawn to scale, showing the relative positions of the components at these various epochs. The arc of the apparent orbit passed over during the sixty years covered by the observations is about 84°. Some of the early measures are very discordant in distance, and so obviously erroneous that it is necessary to reject them altogether in making any investigation of the real orbit. The object

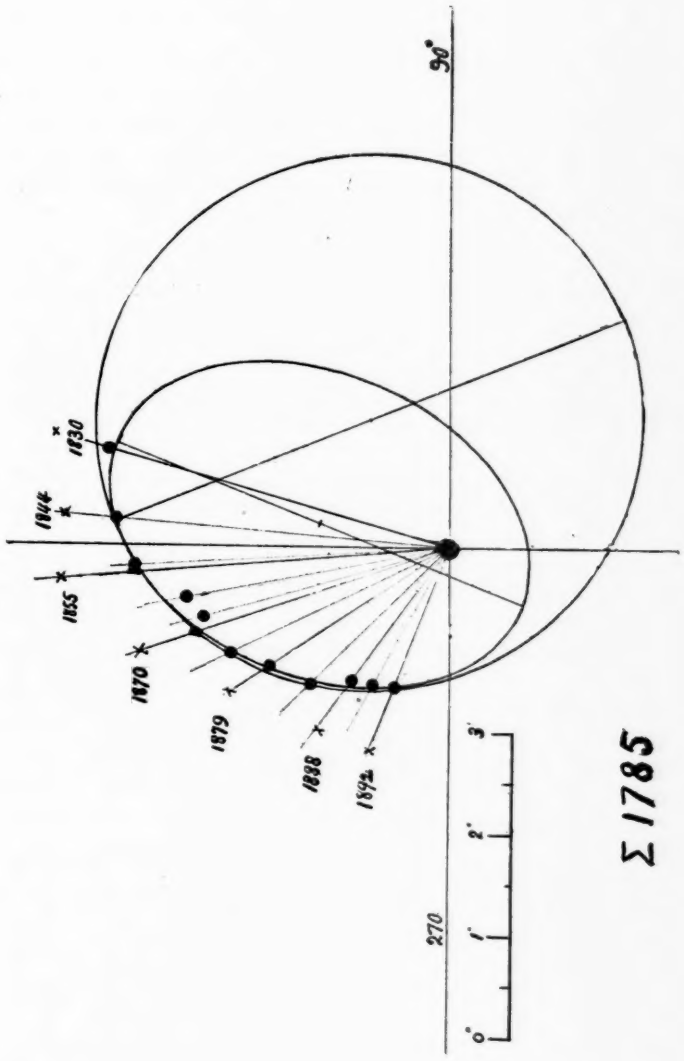
* Communicated by the author.

in collecting these measures was to furnish data for a careful investigation by those who give their attention specially to this department of theoretical astronomy; and I had supposed that the facts were now sufficient to obtain a fairly approximate period.

I have recently examined these measures more carefully with a view of ascertaining the periodic time, or within what limits it is likely to be. I have used, as heretofore, the graphical method. This, for preliminary investigations at least, seems to possess advantages over any other in placing before the eye at a glance the entire case, and showing at once the result of any proposed change in the apparent ellipse. Where this is not so readily seen, one is apt to be misled by an apparently satisfactory agreement between the observed and calculated positions, and to conclude that the result must be the best that is obtainable from the given measures.

The accompanying diagram gives the results of this examination. I have used selected mean results at convenient intervals as shown, which are laid down accurately to scale. The smaller ellipse was then drawn to satisfy equal areas in equal times as accurately as possible. This ellipse gives a period of about 130 years, and an eccentricity of the real ellipse of 0.63, with periastron passage in 1906. It was apparent that with the length of arc discribed, many other and much larger ellipses could be drawn which would equally well satisfy the observations, and, so far as one can tell at this time, give periods as likely to be correct. The larger ellipse was then drawn, and it represents the measures throughout exactly as well as the other. There is no appreciable difference between them in this regard except as to the measures of Struve in 1830, and the difference there is only 0″.11. This is not only an insensible quantity in such a distance, but far below the probable error of his measures. In fact his measures of this pair range in distance from 3″.32 to 3″.57, the mean being 3″.487. If any weight were to be given to the other measures of that time, South 5″.66 (1823.40), Herschel 4″.62 (1830.20) and Herschel 7″.69 (1831.34), the distance of the companion as given by the larger ellipse is still much too small, consequently the ellipse itself. These other measures, however, should not be used for any purpose, and it is far better to rely wholly on the obsevations of Struve.

The second ellipse gives a period of a little over 300 years. The eccentricity of the real ellipse of which this is a projection is 0.56, differing in that respect but little from the other. It is probable


90°
1830
1844
1855
1870
1879
1898
1872
270
0°
1'
2'
3'
Σ 1785

that the first ellipse could have been made a little smaller, so that it appears to be safe to say that the period of this pair is somewhere between 120 and 300 years, and that until a longer arc has been passed over, it will be impossible to say with any certainty what the real time is. For the next ten years the two ellipses are practically coincident, and the measures will not show the extent of the apparent orbit, but the observations of the suceeding decade should be sufficient to confine the error of the periodic time within narrow limits. This will always be an easy pair to measure, for with the shortest time the minimum distance will not be less than 0″.6 and may be as much as 1″.

THE BALANCE ROOF FOR TELESCOPE BUILDINGS.*

CHARLES A. POST.

In the March issue of ASTRONOMY AND ASTRO-PHYSCS I notice an article ay Mr. A. E. Douglass, entitled "The Balance Roof for Telescope Buildings," in which the writer describes a form of roof for telescope sheds adopted at the Boyden Station, Peru, which he thinks will be useful to eclipse expeditions and also to the owners of small telescopes.

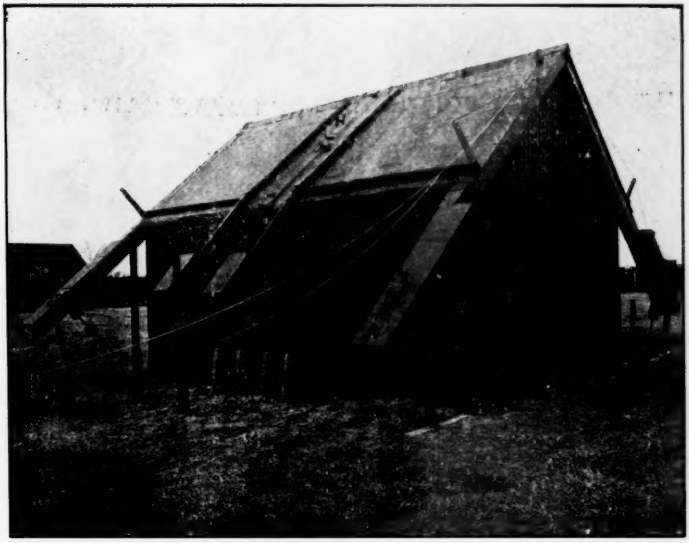
In a matter of this kind any question of priority of invention is of little moment, but as the article substantially describes a somewhat larger structure built, and as far as I know, invented by me, during the winter of 1890–1891, perhaps my three years' experience may also be of interest to your readers. I inclose a number of photographs which were taken during the winter of 1891–1892, and of which copies have been quite widely circulated among my friends. These photographs were shown, and explained by me, at a meeting of the New York Academy of Sciences during that winter, a report of which appears in their transactions.

It will be noticed that my house differs from the one described by Mr. Douglass only in the point of size and in the fact that the roof opens in four sections, instead of two.

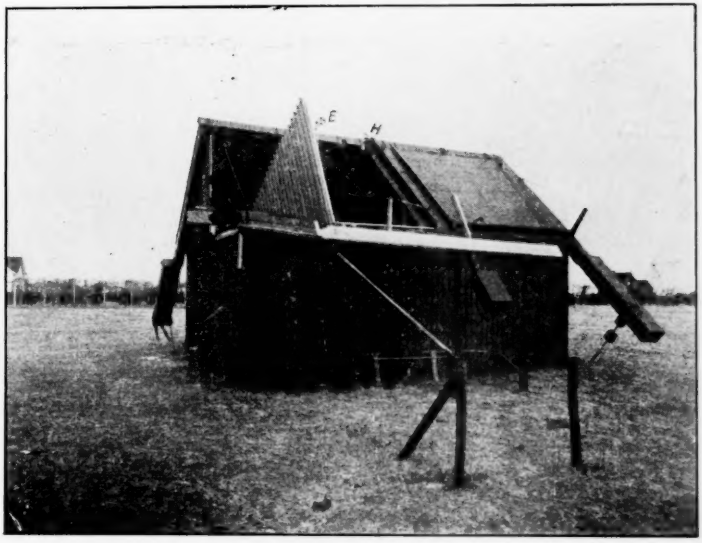
Its dimensions are 20 × 16 feet. This gives me room for a two-inch transit, a sidereal clock, and a dark room for photography, besides affording ample space for a six-inch equatorial.

Referring to the photographs,† No. 1 shows the Observatory closed. A A A are boxes filled with sand, which counterpoise

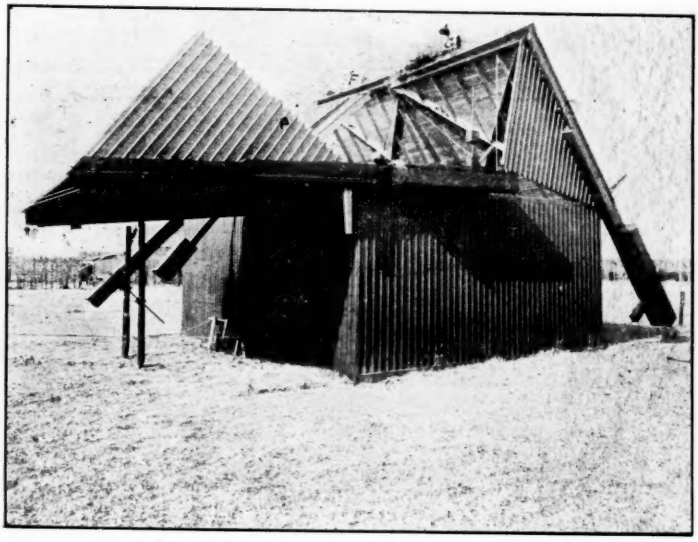
* Communicated by the author.
† Plates XXIII and XXIV following.



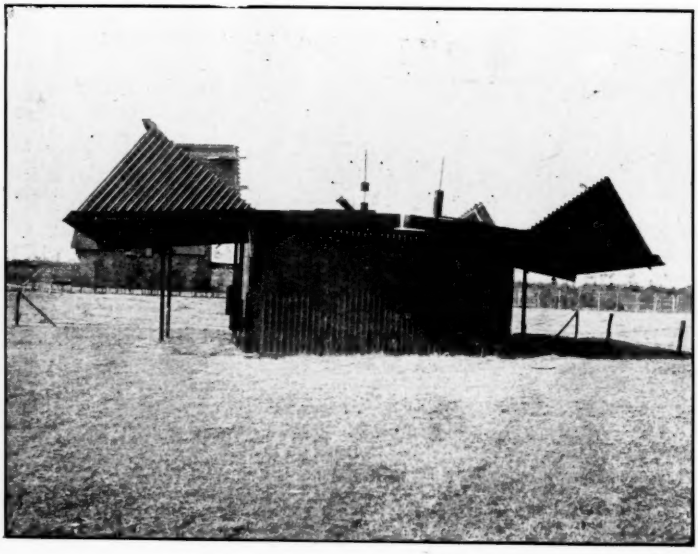
PHOTOGRAPH NO. 1.



PHOTOGRAPH NO. 2.



PHOTOGRAPH No. 3.



PHOTOGRAPH No. 4.

XUM

the weight of the roof. B B B are rods, hinged at the eves, also carrying boxes filled with sand. The use of these weights is "to prevent the severe strain which a roof of such a size would suffer when lifted by one end only." The strain is further taken up by wire rope braces shown in photograph No. 2, marked C. These braces may be set up by screw turnbuckles. D D D are iron rods at right angles to the counterpiose boxes on each of which slides an iron weight, which is held in any desired position by a set screw. The object of these weights is so to adjust the centers of gravity of each section that when an inside clutch, marked E, photograph No. 2, is released the section will assume the position of F and G, photograph No. 3,—from which point it is hauled open by a cord, leading inside of the building. When this cord is released, the section at once assumes the position F again, when a slight pull on another cord causes the roof to close and the clutch to engage.

The house may be used with any one of the four sections open, the others being closed as in photograph No. 2.

The material of the roof is half-inch pine, covered with heavy canvas painted. The canvas comes down over the eves and is fastened to the side of the house over the hinges.

A lip of zinc, attached on one side only, and bent to the pitch of the roof, covers the joint at the ridge, and two small shutters marked H, photograph No. 2, also worked from the inside, cover the opening between the sections.

The building is absolutely weather-tight, and has admirably protected the instruments during two winters. I have also in use a small corrugated iron dome, 12 feet in diameter. In this the snow is sometimes troublesome, but it never finds its way into the structure under discussion.

The beauty of the edifice is perhaps doubtful, but its cheapness and utility are beyond dispute. The cost of the present building was about $275, while I am imformed that a dome of similar size would cost over $2,000.

It is handled with great ease. In fact a child could open and shut it unless the wind was very high, when some strength would be required. The protection to the observer is nearly as good as that of a dome, while the extended view of the sky and the freedom from heated currents of air are decided advantages.

BAYPORT, March 13th, 1893.

ORBIT OF THE BINARY STAR β 416, SCORPII 185.*

PROFESSOR S. GLASENAPP.

In ASTRONOMY AND ASTRO-PHYSICS for December 1891, Professor S. W. Burnham has called the attention of astronomers to a new binary star, β 416, discovered by him in 1876. Although there were given seven positions of the components, they were grouped in such a manner that they were insufficient for the determination of the exact orbit. Last year Professor Burnham measured this star on four nights, and has kindly communicated to me the results of his observations.

The following is a list of the measures of this system:

t	θ	S	Mag.	*n*	
1876.52	240° ±	1″.8 ±	6.0—8.5	1	Burnham
77.53	222 .6	1 .80	7.0—8.5	1	Cincinnati
77.64	224 .4	1 .77	7 —9	1	Russell
88.72	147 .5	1 .89	6.0—7.5	1	Burnham
89.47	134 .1	1 .35	6.4—7.5	3	Burnham
89.63	131 .9	0 .97	6 —8.5	1	Pollock
91.53	82 .3	0 .51	6.9—7.6	3	Burnham
92.38	24 .4	0 .58		4	Burnham

We take the simple arithmetical means of the measures made in 1877 and 1889, and thus obtain six mean places:

1876.52	240° ±	1″.8 ±
1877.58	223 .5	1 .78
1888.72	147 .5	1 .89
1889.55	133 .0	1 .16
1891.53	82 .3	0 .51
1892.38	24 .4	0 .58

From these observations we derive the following elements:

$$T = 1892.00$$
$$\mu = 34.85 \text{ years.}$$
$$\eta = -10°.3293$$
$$\Omega = 104°.3$$
$$\lambda = 300°.7$$
$$i = 45°.4$$
$$e = 0.65$$
$$a = 1''.52$$

These elements represent fairly well all the observations except the first which was only an estimate of the angle and distance at the time the pair was discovered in 1876 with the 6-inch refractor. (See Seventh Catalogue of New Double Stars, *Am. Jour. Sci.* Sept. 1876; and ASTRONOMY AND ASTRO-PHYSICS, X, 489).

The comparison of the observed and calculated positions will be found in the following table:

* Communicated by the author.

t	θ_o	θ_c	$\theta_o - \theta_c$	ρ_o	ρ_c	$\rho_o - \rho_c$
1876.52	240° ±	228°.0	+ 12°.0 ±	1″.8 ±	1″.90	− 0″.1 ±
1877.58	223 .5	224 .1	− 0 .6	1 .78	1 .84	− 0 .06
1888.72	147 .5	146 .8	+ 0 .7	1 .89	0 .96	(+ 0 .93)
1889.55	133 .0	134 .1	− 1 .1	1 .16	0 .87	+ 0 .29
1891.53	82 .3	81 .7	+ 0 .6	0 .51	0 .52	− 0 .01
1892.38	24 .4	24 .0	+ 0 .4	0 .58	0 .39	+ 0 .19

The observed distance in 1888 seems to be too large, and, being only a single observation, I have omitted it in determining the major axis of the real ellipse.

It is to be regretted that in the interval between 1877 and 1888 there are no observations. During this time the satellite described an arc of 76°, and a single observation in the middle of that interval would furnish the data for obtaining a very exact orbit.

This star has a rapid motion, and it is very desirable that astronomers possessing large telescopes should observe it each year. For this purpose I have made the following ephemeris:

Ephemeries of β 416.

t	θ	ρ
1893.44	322°.1	0″.63
1894.44	300 .4	0 .93
1895.44	289 .0	1 .19
1896.44	281 .4	1 .39
1897.44	275 .6	1 .56
1898.44	270 .7	1 .69
1899.44	266 .6	1 .80

Observations of the current year, and of 1894 will be of great value for the study of the orbit. I should be very glad to secure any measures made this year, and will endeavor to ascertain the corrections of the elements.

The place of this star for 1880 is:

$$\text{R. A.} = 17^h\ 10^m\ 47^s$$
$$\text{Decl.} = -34°\ 51'.2$$

ABASTUMAN, Gov't. Tiflis, Russia,
March 1, 1893.

NOTE.—Since sending the elements of the orbit of β 416 to the editors of ASTRONOMY AND ASTRO-PHYSICS, I have received No. 3154 of the *Astronomische Nachrichten*, which contains measures of double stars at the Sydney Observatory. Among these observations are the following measures of β 416 by Mr. Sellors:

1890.600	123°.3	0″.81	7 — 9
.608	120 .6	0 .82	
1890.60	121 .95	0 .81	

I have compared the elements given above with this position, and have obtained:

$\theta_c = 113°.0$	$\rho_c = 0''.72$
$\theta_o = 122\ .0$	$\rho_o = 0\ \ .81$
$\theta_o - \theta_c = +\ 9°.0$	$\rho_o - \rho_c = +\ 0''.09$

We could use these observations for determining the corrections of the elements, as the difference of 9° is not excessive in so small a distance, but it will be better to do this from new observations made during the current year. Such measures will be of great value because the radius vector will describe an arc of 60° since the last position in 1892.

THE PERIOD OF 20 PERSEI (β 524).*

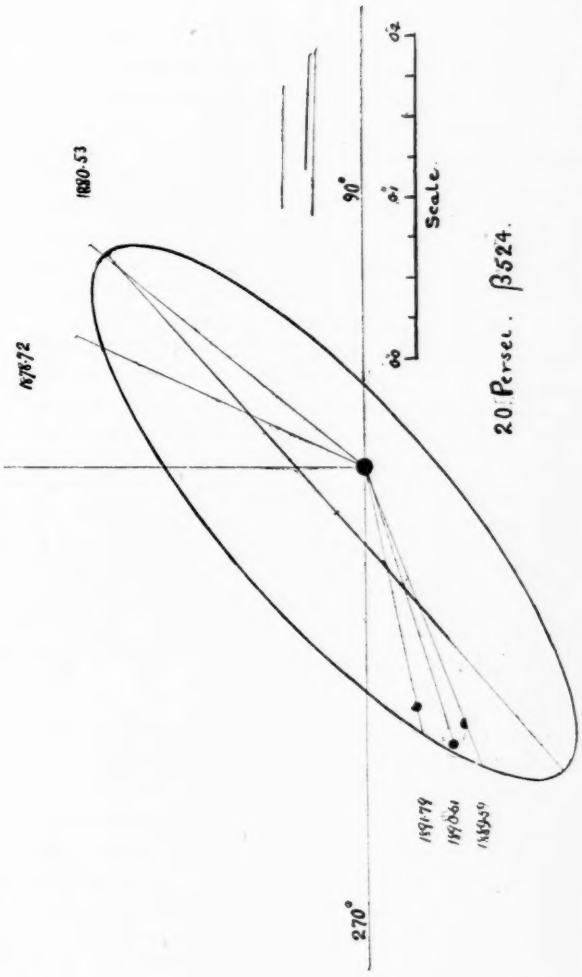
S. W. BURNHAM.

The duplicity of 20 Persei was discovered at Chicago with the 18½-inch refractor of the Dearborn Observatory on February 5, 1878. It was an extremely close and difficult object for that instrument, but I succeeded in getting a fairly good set of measures in that year, and again in 1880. With the exception of a single observation at Madison in 1881, I had no opportunity to measure it again until I resumed double-star work at Mt. Hamilton. It would follow almost as a matter of course that so close a pair would be found to be in rapid motion. All of the most interesting binaries, and these have periods of less than twenty-five years, belong without exception to this class. The notable examples are, $\varkappa$ Pegasi (β 989), δ Equulei (OΣ 535), 85 Pegasi (β 733), ζ Sagittarii (Winlock), β 883, and perhaps β Delphini (β 151), 9 Argus (β 101), and β 416, and certainly many other β pairs whose orbits cannot yet be computed.

The accompanying diagram is made as a preliminary investigation of the probable period of this pair. The measures are too few to give anything more than an approximate value, and it will be at least some years before additional data can be obtained. At all times it will require a large aperture, and an experienced observer to get reliable measures. A pair of this kind is a severe test in both directions.

The following are all the available observations:

* Communicated by the author.


1830·53
1878·72
90°
0·1
0·2
0·0
Scale.
20 Persei. β524.
270°
191·79
190·061
188·950

1878.72	156°.0	"less than 0″.25"	β 4n
1880.53	141 .4	0 .22	β 3n
1889.59	291 .3	0 .17	β 1n
1890.61	287 .6	0 .18	β 3n
1891.79	281 .7	0 .15	β 3n

These measures were all made with the 18½-inch Chicago refractor, and the 36-inch refractor at Mt. Hamilton. The means of the first two sets differ slightly from those given at the time the measures were first published, since I have combined my measure of 1878.909 with the first set, in place of the second, as was done originally when the rapid change in the angle was not fully considered. I have also rejected two single measures with inferior instruments, and the observations of Engelmann in 1883 which are certainly erroneous in angle. His instrument was much too small to do anything with a pair of this kind, notwithstanding his skill as a double-star observer.

The mean of the distance measures in 1878 is 0″.32, but I expressly stated at the time the observations were published that this was too large, and that the real distance was probably less than 0″.25. There is little doubt, when the subsequent measures are considered, of this being the fact, and that 0″.20 would be more nearly correct. The latter value has been used on the diagram. The measures as a whole are very consistent, and apparently accurate, when the extreme difficulty of this pair is taken into account. The largest telescope in the world is none too powerful to properly measure it under the best conditions. The approximate corrections to these measures to make them conform to the apparent orbit shown on the diagram are as follows:

1878.72	153°.8	0″.19
1880.53	142 .5	0 .21
1889.59	294 .0	0 .20
1891.79	279 .5	0 .16

The differences are trifling when compared with the unavoidable and ordinary errors of observation in so close a pair.

This ellipse gives a period of 20.7 years, eccentricity 0.50, and periastron passage in 1883.8. The minimum distance would be but little more than 0″.03, and of course no telescope in existence could possibly show any elongation. It will be a number of years and perhaps a good many, before it will be measured again, but I trust the time will come when some one of the large telescopes of the future will be devoted to the study and measurement of these remarkable systems.

XUM

As a wide double star, 20 Persei has been known since the time of Sir William Herchel who measured it in 1783 (= H III. 60). It is also entered in the catalogue of Sir James South (= S 420), and later went into the *Mensura Micrometricæ* as Σ 318. This companion is much too distant to make any physical relation in the least probable. As a matter of fact, the two stars have been relatively fixed from the first, as will be seen from the following.

1783.58	239°.5	14″.03	H 1n
1829.14	236 .8	14 .08	Σ 2n
1890.61	237 .2	14 .08	β 3n

There are many other measures of this star, but these are sufficient to show that it is devoid of interest so far as motion is concerned.

ON THE FORMATION OF RINGS AS A PROCESS OF DISINTEGRATION.*

DR. M. WILHELM MEYER.

In an earlier article on the fifth satellite of Jupiter, on page 142 f. f. of this volume (*Urania*, 1893) it has already been shown that on the surface of the very small satellite which revolves in the neighborhood of the most powerful center of attraction after the sun, there is a very singular contest between the attractive forces, which on the one side tend to draw a free body to the center of the moon itself, and on the other to draw it to Jupiter. I came, indeed, to the conclusion that the force operating in the direction of Jupiter must certainly be the predominant one. Since this result opens a very interesting cosmological question (to which I shall come immediately) it will be of importance first to show how far the above conclusion is hypothetical.

In respect to this I will take the liberty of referring to my article "A Demonstration of the System of tne Universe," which appeared in the first annual volume of this Journal.† In this it was shown, even to those who know nothing of mathematics, that one is theoretically able to determine with absolute certainty the attraction which is exerted upon any given heavenly body of our planetary system, providing the same has another revolving about it. This revolution is a direct result of the attractive force

* Communicated by the author in German and translated by Miss G. M. Potwin of Carleton College.

† Urania.

of the central body which, as has been demonstrated, in the entire solar system decreases with the square of the distance from the center. At the same time this effect of gravity is directly dependent upon the mass of the body. When one, by observation, finds in a certain case that a satellite falls toward its planet faster than a body at the same distance would fall to the earth, it proves that the mass of the planet is proportionately greater than that of the Earth.

Now since Jupiter possesses, besides the new moon, four previously known satellites whose motions have been quite exactly studied, one can compute with all certainty, not only what attractive power Jupiter exercises upon a body at its surface, but also at any given distance. If we find therefore that at the distance of the first—*i. e.*, the new satellite of Jupiter—a body would fall 1.96 m toward Jupiter in the first second, this is determined with certainty.* Now let us transport ourselves to the surface of the new satellite and imagine Jupiter standing exactly overhead. Every object on the surface will have the impulse in the first second to rush through the above specified distance toward Jupiter; for Jupiter, one would call it a falling motion, for the satellite, it is an upward motion.

The question now arises whether the attraction of the principal planet is greater than that which the mass of the satellite exerts upon an object at its surface. We must make hypothetical suppositions as to the density of the mass of which the moon is formed, and at the same time also as to its size, *i. e.*, its diameter. Now as far as the density is concerned the effects of attraction which the remaining satellites of Jupiter exercise upon each other have shown that the satellites are less dense than Jupiter itself. Now as is well known, Jupiter consists of a mass four times less dense than our Earth; this again is on an average much less dense than many of the heavy substances which appear upon it; for instance, the density of platinum is about four times that of the Earth. Platinum is the densest, the heaviest of all known substances; should the Earth consist of platinum our Moon would necessarily be impelled to fall four times faster toward it. No heavenly body, as far as has been determined, possesses so great a density. Should we assume, nevertheless, that the new satellite of Jupiter posseses the density of platinum, we would give, in all probability, the highest possible maximum limit to its manifestation of power. Under this assumption, however, we

* Later observations by Barnard have given other values for the distance and time of revolution of the new heavenly body, from those first made known and used here. One may rather abide by the old values for the present since the new ones are not yet to be regarded definitive.

can calculate quite exactly how large the new heavenly body must be in order that an object upon its surface shall be drawn just as strongly toward its center as by Jupiter, so that it will be entirely without weight when the principal planet stands in its zenith. It results, that to accomplish this, that the new satellite must have a diameter of 1300 km. The real size of this satellite is, to be sure, not directly measurable. It appears, as is known, as a very small point of light without diameter which, as it seems, could hitherto be made visible in only two telescopes,* of which one, as is well known, is the largest in the world. But again it can be calculated with certainty, that a heavenly body with a diameter of 1300 km. at the distance of Jupiter must have a diameter of nearly half a second of arc, which would necessarily be visible in our good telescopes of modern times.

Since this is not the case, it is to be concluded with certainty that a freely movable object on the surface of the new moon must immediately fly away from it toward Jupiter as soon as the latter rises above the horizon. True, this inference is hypothetical, but it could only be overthrown if the satellite consisted of a much denser mass than we have ever become acquainted with on the earth or in the whole wide universe. The probabilities are therefore many thousand to one, that the new satellite is not able to hold freely movable objects upon its surface.

It is noteworthy that similar circumstances, although, for the most part, not so remarkable ones, exist in cases of all satellites which are to be found specially near their planets. Both satellites of Mars, the first of the old satellites of Jupiter, the first three of the satellites of Saturn, the first two of the satellites of Uranus are all in the same situation, at least upon the supposition that the mass of the satellites is not denser than that of their planets. The following table will show this:

	g. In metres.	Δ	D. Computed km.	D. Obs.
Phobos	0.242	Density of Mars	2550	8.3
Deimos	0.049	Density of Mars	409	7.2
5 Satellites of Jupiter	1.96	Density of Jupiter	20600	.....
5 Satellites of Jupiter	1.96	Density of Platinum	1300	.....
I Satellites of Jupiter	0.354	0.52 Density of Jupiter	7170	3814
II Satellite of Jupiter	0.140	Density of Jupiter	1475	3413
Mimas	0.549	Density of Saturn	12090	513
Enceladus	0.333	Density of Saturn	7330	635
Tethys	0.217	Density of Saturn	4790	989
Dione	0.132	Density of Saturn	2920	941
Rhea	0.068	Density of Saturn	1500	1295
Ariel	0.080	Density of Uranus	1020	.....
Umbriel	0.041	Density of Uranus	524	.....
Titania	0.010	Density of Uranus	123	925
Satellite of Neptune	0.027	Density of Neptune	229	3163

* Telescopes above 18 inches aperture in America have seen this satellite. [Ed.

In this table *g* indicates the space through which a body at the distance of the satellite would fall towards the planet in one second of time.

Under △ the assumed density of the satellite mass is given; the last two columns give the true diameters of the satellites computed according to the assumption more exactly denoted above and those ascertained from photometrical and other measurements.

What will take place upon the surface of these satellites under the above designated conditions? This may be followed up with certainty by computation. With the impulse of bodies, not held at the surface by molecular forces, to fly toward Jupiter, will be combined their motion in the orbit of the satellite; the bodies leave the satellite and scatter themselves along the orbit forming a ring, whose diameter must be less than that of the satellite's orbit and which must resemble in every respect the gauzy, transparent ring which the bright ring of Saturn surrounds. If more and more objects come to it, dust, rock that has crumbled away from the satellite, then the ring will become ever denser and finally resemble the bright rings of Saturn. At the outer edge of this ring, the original satellite will continue to revolve and feed the ring with the objects which have deserted it, It can be easily shown that there will never cease to be such loose objects upon the surface of any given, solid body which revolves around another in our solar system. As long as it is surrounded by water and air, which, however, is no longer possible at this stage, the operation of the atmosphere and moisture produces disintegration. When this no longer operates the alternations between the solar rays and the piercing cold of space, acting all the more powerfully because of the absence of the atmosphere, would by the expansion and contraction of the surface produce fissures, such as already cover our Moon's surface by thousands, and finally large masses of stone which are surrounded on every side by such fissures will be obliged to break loose. This processs of crumbling to pieces, working very slowly to be sure, but incessantly, will constantly reach deeper strata as soon as the upper ones are loosened and all fragments are dispersed over the ring. We can therefore maintain to-day, many thousand to one, *that the fifth satellite of Jupiter began long ago to form such a ring and thus Jupiter possesses at the present time just as Saturn does, a ring at the distance of this satellite.*

Probably, however, the same has too faint a light to ever be perceived. Nevertheless it would not be useless for the astrono-

mers who are equipped with the best telescopes to make observations in this direction. The same is true with respect to the other satellites, and with respect to those of the other systems, which have been already mentioned.

Through the preceding considerations it has been possible to add a new object to the ever widening field of the astronomy of the invisible. It seems furthermore hardly doubtful that the rings of Saturn are the product of the disintegration of several original satellites. This is also indicated by the fact that the profile of the rings extends outward in somewhat pear-shaped form. The densest conglomeration of fragments must indeed be found at the outside according to our observations; at any rate it can be shown that a spherical heap of loose fragments revolving at the specified distance would necessarily form these Saturn rings, even to all the details of their appearance. The rings of Saturn are not therefore, in all probability, to be regarded as a formative stage of a spherical body, which has remained stationary from as yet unknown causes, as has formerly been believed, but are rather to be regarded as a product of the destruction of such a body.

If we assume the Kant-Laplace hypothesis of the origin of worlds to be a fact then it can be concluded from the mere existence of the fifth satellite of Jupiter at the observed distance, that it has approached the planet by a considerable distance since it sprang into existence. That is to say it is impossible with the pre-dominating gravity of the planet and at this distance, that there should take place a condensation of the matter of the original ring into a satellite which is not able to retain its surface portions through its own power.

There exist as is well known still some other reasons which make a gradual approach of the secondary bodies toward the centre of their system highly probable. There is especially the meteor-dust which, ever present in space, opposing a constant resistance to the motions of the heavenly bodies, compels them to fall slowly toward the attracting centre. If this assumption is right one sees how this most invisible and volatile agent in the universe forms one of the principal factors for the disintegration and destruction of the heavenly bodies. It leads the bodies, in the first place, up to that limit at which the attractive power of the planet begins to loosen parts of its surface. Here destructive activity sets in, and in most cases long before the satellite could have fallen upon its planet the dissolution of the satellite into the ring will have become complete. Then, the meteor dust, which

was the cause of this interception and consequently also of that fatal fall, at the same time guards the planet from the terrible catastrophe of the precipitation of the satellite mass as a whole. The satellite mass only falls back upon the mother body in the course of many thousands of years and in the form of quite tiny fragments, falling stars and meteors. Our moon upon which a body falls 0.829 m. in the first second, would begin to dissolve into a ring at the distance of 2.43 Earth's radii from the Earth's centre. It is not impossible that the brightness of the zodiacal lights proceeds from a ring of small meteors which are the product of destruction of a former satellite of the Earth.

Finally the noteworthy fact may be referred to, that all of those satellites of our planetary system which are in all probability already inside the zone in which the disintegrating activity of gravity begins, are remarkably small, from which fact it is to be supposed that the disintegration is already considerably advanced. The only exception to this is the hitherto so-called first satellite of Jupiter, although it is the smallest among the four old satellites. It is, however, doubtful whether the assumed density, equal to half that of Jupiter, corresponds to the reality; should it be once again as dense, thus equaling the density of Jupiter, the satellite is found yet outside the destructive zone.

The craters of our Moon also give a support to the correctness of the view here presented. That is to say, if the satellites really disintegrate into dust rings and from these gradually precipitate fragments upon the solid surface of the planet, then formation would arise from the deposited matter which, as experiments prove, would bear a striking resemblance to the craters of the Moon. The objection is done away with which has been raised against the old meteoritic hypotheses, as to the formations of a Moon's craters, that the great number of the specified formations on our satellite can hardly proceed from those sporadic meteorites, as they occasionally meet our Earth. Such ring meteorites can, and must, in most cases appear in masses and produce whole showers, as certain districts of the Moon's surface seem to show. Since Dr. Gilbert of Washington quite recently expressed a very similar opinion as to the origin of the Moon's craters (Publications of the Astronomical Society of the Pacific, vol. IV., No. 26), I will take the liberty of referring to the fact, that, agreeing perfectly with this gentleman, I have already expressed this opinion in my popular book "Die Königin des Tages," pp. 126 u. f. (Teschen, 1885), and also in the articles "From the Earth to the Moon," which have repeatedly appeared in the *Urania*. (Sammlung populärer Schriften, published by the Urania Company, No. 1, pp. 21 u. f).

XUM

THE EVOLUTION OF DOUBLE STARS.*

C. H. DARWIN.

The essay which we review is a dissertation for the doctorate of philosophy of Berlin, and the author, Mr. See, is an American, although he writes in German.

The component stars in double systems appear to be usually of comparable magnitudes, and are found to move in highly eccentric orbits. This case the author holds to be the normal one, whilst the solar system, with its one preponderant mass, and its nearly circular orbits, would be exceptional.

He attributes the observed high eccentricity of orbit to the influence of tidal friction, and accordingly the greater part of the paper is devoted to the consideration of the results which will ensue from the supposition that each of two bodies raises in the other tidal disturbances, which are subject to frictional resistance.

If the rotations of the two bodies differ in speed, the problem is an insoluble one, without some postulate as to the law of the frictional resistance. The author is, however, of opinion that sufficient insight may be gained from the solution in the case where two equal bodies rotate with equal speed. This opinion seems justifiable, but it might have been well if the dynamical stability of equality of rotations had been explicitly pointed out. That there is such stability is clear from the consideration that, if one of the bodies rotates more rapidly than the other, it is subjected to a more rapid retardation of rotation, and there is accordingly a tendency towards the restoration of equality.

The influence of tidal friction on the elements of the orbit of a satellite and on the rotation and obliquity of a planet have been investigated in my several papers, and Mr. See here adapts my conclusions to the case of the double tidal friction of two stars. The adaptation is not difficult, for whilst, the rate of change in the rotation of each star remains the same as though the other did not rotate, the rates of change of the elements of the orbit are exactly doubled. Mr. See has than redrawn the curves which exhibit the gradual transformation of the system, and as might have been expected, finds them to have features closely similar to those of my curves.

The generality of these solutions is limited by the supposed

* Die Entwickelung der Doppelstern-Systeme. Von. T. J. J. See. 60 pp. (Berlin: R. Friedländer und Sohn, 1893). From *Nature*, March 16, 1893.

smallness of the eccentricity and of the inclinations of the orbit and of the two equators to the plane of reference. The author, however, then passes to a second case, which is more special in that the equators of the stars remain coincident with the plane of the orbit, but which is more general in that the eccentricity is not treated as being necessarily small. The object is to obtain a numerical solution of the following problem:—Two equal stars, each of three times the Sun's mass, revolve in a nearly circular orbit at a distance equal to that of Neptune from the sun, and the rotation of each star is nearly equal to its orbital motion; it is required to find the greatest mean distance and the greatest eccentricity of orbit to which the system will change under the influence of tidal friction.

Mr. See solves this problem by methods analogous to those which I have employed, and finds that the mean distance will increase from 30 (Neptune's distance) to 50, and that the eccentricity will increase from an assumed initial value of one-tenth to a maximum of about three-fifths, which is attained a little earlier than the maximum of mean distance.

It may be remarked that these results can only be very rough approximations to the truth, because the calculation is conducted on the supposition that the moment of inertia of each star is the same as that of a homogeneous sphere of the same mass and radius, whereas it is obvious that the stars would really be highly condensed spheroids of great oblateness.

It is to be regretted that the calculation has not been repeated with variations of the assumed initial conditions. It is easy to see that a change in the assumed degree of concentration of the stars would give very different results. Supposing, for example, the stars had had only half the diameter assumed, the rotational moment of momentum would have had a quarter of its value in Mr. See's example. Now the enlargement of orbit is due to the transference of rotational to orbital moment of momentum, and thus the transferable moment of momentum would only have amounted to one quarter of its former value. But the orbital moment of momentum varies as the square root of the mean distance and hence the enlargement of the orbit could not have been so much as one-sixteenth of its former value. We may feel sure that the increase in the eccentricity of orbit would also have been largely reduced.

Notwitstanding this criticism, it appears to me that Mr. See fairly establishes the proposition that a high eccentricity is explicable by means of tidal friction.

XUM

Turning, then, to the question of the relative masses, of double-star systems, Mr. See remarks with justice that the comparable brightness of the components renders it highly probable that the masses are also comparable, and he sees in certain results of M. Poincaré and of my own an evolutionary explanation of this fact.

Jacobi first showed that an ellipsoid of homogeneous fluid with its three axes bearing to one another proper proportions, is a figure of equilibrium when it rotates about its smallest axes with a proper angular velocity. M. Poincaré next showed that if the length of the Jacobian ellipsoid exceeds the breadth in a certain ratio, the equilibrium becomes unstable, but that there is a stable figure which may be described as a Jacobian ellipsoid with a furrow nearly round the middle, so that it resembles an hour-glass with unequal bulbs. If we trace the further development of the hour-glass we find its neck gradually thinning, and finally rupturing, the figure of equilibrium, henceforth consists of two detached masses.

My own attack on this problem was from the opposite point of view, for I endeavoured to trace the coalescence of a pair of detached masses, so as to form an hour-glass or dumb-bell.

Mr. See reproduces the figures illustrative of both these investigations, and remarks that they both show that when there is gradual detachment from a rotating figure of equilibrium, the detached portion will not normally be a ring, but that there will ensue two quasi-spheroidal masses of matter of comparable magnitude. He also remarks that if the fluid be heterogeneous, the ratio of the masses will be much smaller than when it is homogeneous.

In the discussion of these figures of equilibrium the wording of the essay appears a little careless, for it might naturally be supposed to mean that increase of angular velocity is a necessary concomitant of the rupture of the neck of the hour-glass. Now it is a somewhat paradoxical fact that, with constant density, the longer elongated figures of equilibrium rotate more slowly than the shorter ones, and it might therefore seem that the rupture of the neck should go with retardation of angular velocity. But it is the value of the square of the angular velocity divided by the density which determines the length of the elongated figures, and thus increase of density tells in the same way as retardation of angular velocity. In the history of a nebula the only condition for rupture which can be specified is that of contraction.

The probability of this view of the genesis of double stars is strikingly illustrated by a number of drawings by Sir John

Herschel of various nebulæ. The great similarity between Herschel's nebulæ and the theoretical hour-glass is obvious. It may be hoped that in the book which Mr. See promises he will also illustrate this point by photographs.

Annulation is usually accepted as the mode of separation in the nebular hypothesis, but, as already stated, this is held by Mr. See to be exceptional. He thus regards the ring of Saturn as being exceptional in its history as it now is in appearance. Where he maintains that Saturn's ring will never coalesce into a satellite, he might with advantage have referred to the remarkable investigations of M. Roche,* who showed that a satellite would be torn to pieces by tidal action if it revolved at a distance of less than 2.44 times the planet's radius. We may here note the interesting fact that whilst Saturn's ring almost touches "Roche's limit" on the inside, the Martian satellite, Phobus, and the fifth satellite of Jupiter† almost touch it on the outside.‡

In order to prove his thesis as to the highness of the excentricity and the comparability of masses, Mr. See gives a careful table of the observed elements of the orbits and of the relative brightness of seventy-three pairs of double stars. The values of the elements are of course open to much uncertainty, but the mean eccentricity, which is found to be .45, must lie near the truth. In the few cases in which the masses have been determined, they are found to be comparable, and the comparability of the brightnesses confirms the generality of this law. Thus the facts of observation agree with our author's ideas.

Mr. See must be congratulated on having written an essay of great cosmogonical interest, and although his theory may never be susceptible of exact proof, yet there is sufficient probability of his correctness to inspire us with fresh interest in the observations of double stars.

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* "Acad. des Sciences de Montpelier," vol. i, (1847-50), p. 243. See also Darwin, *Harper's Magazine*, June, 1889.

† The values given by Barnard (*Nature*, p. 377) make the distance 112,000 miles, and Roche's limit 107,000 miles.

‡ It is proper to warn the reader that Roche's limit depends to some extent on the density of the planet. For the Sun it will be about one-tenth of the Earth's distance from the Sun. Thus a body of planetary size cannot move in a highly eccentric orbit, so that its perihelion distance is one-tenth, without being broken up into meteorites; and conversely a flight of meteorites with less than the same perihelion distance can never coalesce into a planet.

XUM

Astro-Physics.

NOTES ON SOME RECENT OBSERVATIONS OF NOVA AURIGÆ.*

W. W. CAMPBELL.

(*a*). On p. 56 of the current volume of ASTRONOMY AND ASTRO-PHYSICS Herr Von Gothard compares the intensities assigned by him and by myself to the lines in Nova Aurigæ's spectrum, and attributes the striking differences in our estimates to the different absorptive powers for violet rays of his instrument and mine. The true explanation is quite another one.

It has been pointed out (in A. AND A.-P. for November, p. 803) that the relative photographic brightness of lines in different parts of the spectrum cannot be obtained from my plates, since rays of only one wave-length enter the slit properly. Hence my estimates, quoted by Gothard, are *visual;* and, as stated in A. AND A.-P. for October, p. 716, the intensities of λ 4466 and λ 4336, which were not visible to me, "were estimated from the photograph by comparison with the line λ 4360." Herr Von Gothard's intensities are *photographic*, and based upon the curve of sensitiveness of an orthochromatic plate which would be shown by a photograph of the solar spectrum to be very peculiar.

On my plates, "by far the brightest line . . . is that at λ 4360. It is eight or ten times as intense as the Hγ line at λ 4336" [A. AND A.-P. for November, p. 820]. On Herr Von Gothard's plates these two lines are not separated, owing to the small dispersion used. They combine to form his line λ 434, "which is the most intense line of the entire spectrum," as the line λ 4360 is on my plates.

The curious curve of sensitiveness of his plates is shown by the absence from all his results of the lines λ 496 and λ 486, which we know to exist prominently in all the spectra photographed and in a region slightly actinic with ordinary plates; whereas λ 5750 in the yellow is successfully photographed, though it is no brighter visually than the blue line λ 486.

Herr Von Gothard has apparently taken the line λ 434 as the zero point for measurements of his plates. If on the Nova plate he assign to that line the wave-length 4360, the apparently erroneous position of Hδ (λ 4077) will be explained.

(*b*). I have re-measured the position of the yellow line, in connection with some work on the Wolf-Rayet stars, and have obtained for it:

* Communicated by the author.

1893 Feb. 15, λ 5752, compared with mg λ 5712.
" 18, 5751, " " hg 5769.

(*c*). In the *Observatory* for January the Astronomer Royal for England called attention to a remarkable decrease in the brightness of Nova. It "was noted as invisible in the 10-inch finding telescope about Oct. 7th by Mr. Turner, Oct. 22d by Mr. Davidson and Oct. 25th by Miss Everett. This would imply that the Nova is below the 14th magnitude."

The Nova was observed by me visually with the four-inch finder on Oct. 12th, 19th and 22d, and spectroscopically with the grating on the 12th and 19th, and no significant decrease of brightness over that of August and September was detected. It was observed by Professor Barnard with the 36-inch on Oct. 21st, 23d and 25th; and in his opinion, since August the object has "as a whole remained essentially constant in its light." Estimates made at Oxford on Oct. 5th, 7th and 10th placed the magnitude at 9.5; on Oct. 19 at 9.7; on Oct. 25 at 9.8. It is impossible to harmonize the observations made here and at Oxford with those made at Greenwich on the same nights, unless, indeed, the Nova varied 4.5 magnitudes in a few hours. Such a variation must have great significance in any theory of the Nova, and it is unfortunate that we know so little of the light-curve on those nights.

(*d*). The following wave-lengths for the chief nebular line are additional to those already published:

Date.			λ	Velocity.
1892.	Dec. 13*	1st order grating	[5004.18]	[— 107 miles]
	" 14*	1st " "	[4.02]	[— 109]
	" 14*	2d " "	[4.22]	
	" 18*	1st " "	6.38	— 29
	" 18*	2d " "	6.16	
1893.	Feb. 10	1st " "	6.34	— 30
	" 10	2d " "	6.14	
	" 14	1st " "	5.90	— 33
	" 14	2d " "	6.41	
	" 27	1st " "	5.72	— 52
	" 27	2d " "	5.60	

Observations marked thus * were made by Mr. S. D. Townley in my absence from the Observatory. In his notes I find: for Dec. 13, "wind nearly 60 miles per hour," observations few and discordant; for Dec. 14, "wind 50 miles per hour," observations discordant; for Dec. 18, "very little wind," observations very accordant, spectroscopic adjustments tested next morning on Sun. The observations of Dec. 13 and 14 are entitled to very small weight. It is pretty certain that the wave-lengths were considerably larger in the interval of nearly two months when no observations were secured.

In any discussion of these observations it is necessary to take into account the difficulty of accurately locating the centre of a line so broad and diffuse as this one is. On two nights, in August and September, I suspected that the appearance of the line was changing, but was not able to confirm my suspicions [see A. AND A.-P. for October]. The error in the result for any night can scarcely exceed ten miles a second, and there can be no doubt of the reality of the change of wave-length. If it is due to orbital motion, a very unusual orbit would be required to satisfy the observations.

(*e*). Dr. Roberts' photographs of the region about Nova Aurigæ are certainly very interesting in that they show the probable non-existence of a *large* nebula, such as required in Dr. Seeliger's hypothesis. But to me it seems impossible that the 21″ or 13″ image of Nova should be seriously expected to prove anything concerning the 5″ nebula so easily visible in the 36-inch telescope, except that its light falls more largely in the non-actinic part of the spectrum than does that of the visually fainter star whose photo-images were 23″ and 16″ in diameter. I would suggest that the value of the evidence be tested by photographing in the same way some planetary nebula that resembles the Nova in magnitude and structure, as for example, N. G. C. 6790, and comparing the image of the nebula with that of a star slightly fainter [visually].

MT. HAMILTON, 1893, March 21.

ON THE ORIGIN OF SUN SPOTS.*

EGON VON OPPOLZER.

The question of the origin of Sun spots has not yet been satisfactorily solved; for every theory of Sun spots must be considered incomplete, unless it explains phenomena so intimately connected with the nature and origin of the spots, as are the periodicity and heliographic distribution of the spots. As, however, Young says in regard to the problem, this cannot be said of any of the theories thus far proposed; but every such theory must in addition be considered incomplete, unless it also makes plain the peculiar law of rotation of the Sun's latitudes, which goes hand in hand with the number and distribution of the spots. This discovery is due to Spoerer, but is too little taken account of in the

* Translated from *Astronomische Nachrichten* No. 3416.

numerous theories of rotation. The result which Spoerer has derived from the discussion of spot observations (1861-71) is as follows:*

"The year 1866, which preceded the year of spot minimum, is remarkable from the fact, that at that time the usual angular rate of rotation almost entirely disappeared. . . . Any one who observes in a single year in which such circumstances exist, as in the year 1866, must consider it permissible to combine all the rotation angles into a mean value, . . . so that in the two years preceding the spot minimum the mean values of the angles of rotation give, with remarkable uniformity, nearly the same curve. . . . The evidence, that within the eleven-year period characteristic differences (before and after the minimum) present themselves in the annual curves of the mean values of the angles of rotation obtained for different latitudes completely excludes the idea, that there might be special periods of rotation for different zones on the Sun's surface; and we are led to attribute the periods of rotation obtained from spots to currents, which are at least as variable as the number of the spots and the distribution of the same."

This result, so important for astro-physics, is found again in the discussion of the succeeding periods, although not to quite so marked an extent. Unfortunately Dunér's observations of the rotation of the Sun† extend over a space of time of too small solar activity (3 years before the minimum) to further corroborate this phenomenon; but they seem to be favorable to it. When we consider the remarkably small probable error of Dunér's observations—they reach at the highest 0.03 km—I believe the following conclusions positively confirmed: Dunér forms for the velocities‡ of the individual zones the combined mean of the values for the years 1887, 1888, and 1889. If, now, we compare the means for the individual years with this combined mean, we obtain the following differences (mean combined mean):

SPOT ZONES.

φ	0.4	15.0	30.0	45.0	60.0	74.8
1887	+ 0.01	0.00	− 0.02	+ 0.02	+ 0.03	− 0.02
1888	− 0.08	− 0.08	− 0.03	− 0.03	+ 0.04	+ 0.05
1889	+ 0.07	+ 0.07	+ 0.06	0.00	0.00	− 0.02

We see immediately from this table, that the year 1887, which is furthest from the minimum year, coincided with the general

* Spoerer, Publ. der Astr. Gesellsch. XIII, pp. 148-153; 1874.
† Dunér, Nov. acta reg. soc. Ups. Ser. III, Vol. XIV, 1891.
‡ In km per second.

mean; all the differences lie within the limit of error; the year 1888 shows large negative differences, *i. e.* smaller velocities, chiefly in the spot zone; correspondingly, in the year 1889, the year before the minimum, there were large positive differences, *i. e.* greater velocities, and these only in the spot zone; in the higher latitudes, on the other hand, the same circumstances prevail as existed in all latitudes in the year 1887, circumstances which tend to limit the velocities. Exactly the same appearances as those which the iron vapor at the surface of the Sun here shows with reference to its motion about the Sun, appear in the motions of the spots; also, if at the time of minimum the succession of spots begins in higher latitudes, other circumstances immediately arise there, and such as demand the ordinary rate of angular rotation, and this fact strikingly suggests how intimately the motion of rotation of the Sun's latitudes is related to the question of heliographic distribution, and gives us at the same time a clear indication as to where we must seek the cause of the rotation peculiar to the Sun, and wherein that cause consists, namely, in a limitation of the velocities.

After these preliminary explanations we will pass to a consideration of the character of the spots. The spots, and especially their nuclei, are masses of gas and vapor, which, by reason of their lower temperature, strongly absorb the light radiated by the photosphere. This shows itself in the spot spectrum, not merely by the strengthening and widening of many lines of the solar spectrum, but also by the appearance close to one another of countless lines, first discovered by Young* and later by Dunér,† which are not seen in the ordinary solar spectrum on account of their delicacy and faintness, and whose constancy of position leads us to conclude that the absorbing mass is very quiet, so that for this reason Dunér is inclined to prefer Secchi's to Faye's theory. That the spots are sunken, or, and this is enough for our question, may be sunken, into the photosphere may be considered a sufficiently established fact. Even Spoerer in his latest publication of Sunspot observations,‡ p. 427, is lead to assume this when he writes: "It must be assumed for these spots that the visible surface of the nucleus lay deeper than is the case on the average, unless in this and other cases we ascribe the deviations to errors of observation or to the configuration of the spot." If we combine all these results, we

* Young, Amer. Jour. of Sci., Ser. III, Vol, XXV.
† Dunér, the above mentioned memoir, p. 12.
‡ Spoerer, Publ. d. Astroph. Obs. z. Potod. Bd. IV, St. 4; 1886.

obtain the following picture of a spot: In the condensed vapors of the photosphere is a cavity or depression, upon the floor of which lies a layer of cooled vapor. How is it then, in general, explainable that a cavity of this sort is formed of several hundred miles extent in this condensed mass of vapor, and can besides continue for months, and further that such a difference in temperature between this cooled layer of vapor and the photosphere can be sustained so long? Such a cavity is only conceivable when gases or vapors of higher temperature exist in it which hinder a condensation of the photospheric vapors. In a spot, then, there must be an extreme reversal of temperature; this is confirmed by the frequent diminution of the lines, which appear in the higher regions of the chromosphere, and their frequent reversal; it is confirmed by the recent observations of Frost, that spots may occasionally be warmer than the surrounding photosphere.* Above the layer of cooled vapor there prevails a heat which is abnormal considering its height above the surface of the Sun; this may be explained readily and only by an atmospheric current flowing down from the chromosphere upon the photosphere. The action of such a current may be completely explained by recent meteorology; especially the fundamental researches of Hann† on this subject which I shall follow thought by thought. From these we have the following results with reference to an ascending current of air:

The mean temperature of the column of air in which the upward motion takes place is far above the usual average; the air within is of extraordinary clearness and dryness. At a certain distance from the Earth's surface the ascending motion must naturally cease, and be transformed into a correspondingly slow horizontal motion; in this part of its path the air is cooled by radiation of heat, which is unusually favored by the clearness and dryness of the upper strata of the air. It is in consequence of this great fall in temperature that those dense clouds are formed, which under such circumstances occupy the lower strata. In the descending column of air the atmospheric pressure is abnormally high.

Have we not in the case of the Sun a nearly complete analogue of the above explanations? The clearness of the photosphere, the quiet of the deeper strata, and their greatly lowered temperature. Since it has also been satisfactorily proved by observa-

* Frost, A. N., Bd. 130, No. 3105-06, p. 143; 1892.

† Hann, Zeitschr. f. Meteorol., Bd. X, p. 210, 1875; Bd. XI, pp. 129-135, 1876; Denkschr. d. W. Ak. d. Wiss. Bd. LVII, 1890.

tion, that a motion directed toward the spot takes place from above,—Spoerer himself has in fact, observed numerous instances in which such currents have been drawn directly toward the spot*—we seem to be limited to some such explanation of the spots, especially when, in addition, we consider that on the Sun these energies must be of much greater intensity than on our Earth. The greater portion of the radiation from the photosphere is absorbed by the gases and vapors lying over it, above all the deeper lying metallic vapors with their numerous lines taking the principal part in this; the upper strata of the chromosphere consists of gases and vapors of relatively little power of absorption; among these are helium, hydrogen, sodium, and calcium. A current which consists principally of these latter substances, and carries with it vapors at a very high temperature only, *e. g.* iron vapors, I will call dry, in contradistinction from such currents as are saturated with vapors, and which I shall call saturated currents. If, now, a saturated current thus flows upward from the photosphere into the chromosphere, and naturally sinks down again somewhere as a dry current, it will have a much higher temperature than it had when it left the photosphere; this results because in its ascent which is naturally accompanied by cooling it must continually condense vapors, which thereby transfer to it their condensation-heat and thus greatly check its cooling.† Descending currents are, therefore, always associated with great heat and dryness, since the vapors which they may happen to contain, being at a high temperature only, exert no influence worth mentioning. When, therefore, such a current strikes the photosphere and remains there, it will, in consequence of its much higher temperature, free the vapors which are there condensed, and by its continual inflow produce great dryness along its path; the consequence of which will be to cause a clearing of the photosphere in this place, which must resemble a funnel shaped cavity; finally the vertical current will branch into horizontal currents, and beneath these places of branching, where the gases must stagnate, we have now conditions suited to a powerful radiation; at this point a considerable fall in the temperature ensues, which is proved by the layer of gas or vapor forming a nuclear spot. The sides of the tunnel also naturally radiate heat, although to a much less extent, since they are inclined to the surface of the Sun, and form the penumbra of the spot. I

* Spoerer, Publ. d. Astroph., Obs. z. Potsd., Bd I, St. 1, No. 1, p. 79: 1878.

† A saturated moist ascending atmosphere undergoes a diminuation of temperature of 0°.54 for every 100 m at 10°; a dry atmosphere one of nearly 1°, thus nearly double that of a moist one.

will not here further consider how observations made directly and with the spectroscope agree with these ideas; this will be done in another place.

We have really completed the solution of the problem given, to explain the origin of the spots; I may however be further permitted to point out how this theory of the Sun spots fulfils every demand made upon it: it explains, in fact, the rotation and heliocentric distribution of the spots. If we assume the Suu spots as the domain of the descending currents, which naturally must ascend somewhere else, and further consider that they arrange themselves in zones, this suggests a common origin, which may be supposed to be at the equator, but in reality is not to be sought there but in the polar regions, since a zone of spots in lower latitudes may reach clear to the equator, while in the the higher latitudes—and this is always the case near the time of minimum—a new vigorous succession of spots is already begun. Thus ascending currents similar to those at the equator in our atmosphere, prevail at the polar regions of the Sun and flow at a certain elevation in long extended spirals toward the lower latitudes where they appear as east winds, and when they descend, as spots. Until a branch current from the pole reaches the lower latitudes, months, perhaps years, elapse; this depends on the velocity of the upward flow. If we suppose the upward flow at the poles alternately increasing and decreasing in intensity, we have, as may easily be seen, the following as the origin of the heliographic distribution of the spots: If, at the time of minimum, the intensity is increasing, the spots will at this time sink in higher latitudes; with the constantly increasing intensity the lower latitudes become scattered over with spots; the spot maximum intervenes, and the current may be almost destroyed, while the east winds still circling about the Sun will sink finally as minimum spots in the neighborhood of the equator; meanwhile the series of phenomena has already begun anew. This upward flow thus explains the heliographic distribution in an extremely simple manner, and at the same time explains the individual rotations of the different latitudes on the Sun. The currents flowing from the poles are accompanied by small velocities, and consequently small angles of rotation, since according to Zoeppritz* and Wilsing† friction in the lower strata has very little influence, and they also, on this account, continue for a long time; if however they have revolved about the Sun several times, an influence will be exerted,

* Zoeppritz, Wied. Ann. N. F. Bd. III, p. 582, 1878.
† Wilsing, A. N., Bd. 127, No. 3039, p. 233, 1891.

such as to cause the east winds of the lower latitudes to show a greater daily angle of rotation than those arising in higher latitudes, which were much less displaced by friction.* The appearances on the Sun's surface also suggest with great probability the upward flow existing at the poles. The polar regions are the calms of the Sun; the often gigantic dimensions of the cloud protuberances, principally in the neighborhood of the poles, continue during a whole revolution, and appear to favor this theory. Especially may this be said of cloud protuberances lying in a horizontal direction, the average height of which exceeds that of all the others which float freely above the photosphere and without visible connection are renewed from beneath, though here and there united by little columns. If the higher temperature of the poles is not the origin of this polar current the latter must at any rate increase the polar temperature. The poles at certain times thus become warmer than the equatorial regions; this, if it can in general be demonstrated, will be most pronounced at the time of minimum, when the succession of spots begins in higher latitudes. This statement is supported by the otherwise wholly enigmatic behavior of the chromosphere, which at the time shows "accumulations" at both poles, while the year before the minimum it shows a uniformity in height extending over all latitudes. By this theory of Sun-spots the problem of the rotation of the Sun, that of the number of the spots, and also that of their heliographic distribution, seem to be reduced to a single problem, namely, that of a periodic upward current in the polar regions. The many further evidences of the ideas here merely suggested, and the further results to which the theory has led me with reference to the constitution and temperature of the Sun, and the nature and constitution of the different protuberances, I propose to present in a work soon to be completed.

DISTRIBUTION IN LATITUDE OF SOLAR PHENOMENA OBSERVED DURING THE FOURTH QUARTER OF 1892.†

P. TACCHINI.

The following are the results on the distribution in latitude of solar phenomena observed during the months of October, November, and December, 1892:

* This is the same idea as that which forms the basis for Zöllner's rotation theory.

† Communicated by the author.

Latitude.	Prominences.		Faculæ.		Spots.	
90 + 80	0.000					
80 + 70	0.015					
70 + 60	0.105					
60 + 50	0.049					
50 + 40	0.016	0.399	0.004			
40 + 30	0.051		0.026		0.010	
30 + 20	0.078		0.086	0.431	0.019	0.442
20 + 10	0.040		0.177		0.250	
10 + 0	0.045		0.138		0.163	
0 − 10	0.044		0.056		0.106	
10 − 20	0.082		0.203		0.202	
20 − 30	0.100		0.211	0.569	0.221	0.558
30 − 40	0.138		0.086		0.029	
40 − 50	0.074	0.601	0.013			
50 − 60	0.125					
60 − 70	0.036					
70 − 80	0.002					
80 − 90	0.000					

The prominences, faculæ and spots have thus been more frequent in the southern hemisphere; metallic eruptions have been wholly absent, at least according to our observations. This is certainly a peculiar condition, for it is rare that spots and faculæ are so numerous without accompanying eruptions. The spot groups have had their maximum of frequency in the zones $(+ 10° + 20°)$ and $(- 10° - 30°)$, exactly as in the preceding quarter, and in accord with the faculæ. The prominences, on the contrary, present their maximum of frequency in zones more distant from the equator, where we have observed neither faculæ nor spots.

R. Osservatorio del Collegio Romano,
Rome, March 9, 1893.

ON THE DISPERSION OF AIR.*

C. RUNGE.

The wave-lengths of light are generally given for air of a certain temperature and a certain pressure. Rowland's standard wave-lengths, for instance, are given for air of 20° Celsius and a pressure of 760 mm. of mercury, and all wave-lengths that are deduced from these by interpolation or by the method of coincidences also refer to the same temperature and pressure. With the change of the density of the air the wave-lengths change inversely proportional to the index of refraction for each particular color.

Now it is necessary for some purposes to take this change into

* Communicated by the author.

account and to reduce the wave-lengths to vacuo by multiplying them by the index of refraction that corresponds to them. The laws, for instance, that govern the distribution of lines in the spectrum of an element must be independent of the alterations, which the wave-lengths undergo on account of the particular temperature and pressure of the air in which they are observed. Thus in many spectra there have been observed a number of pairs of lines, whose wave-lengths λ_1, λ_2 give the same value of $\frac{1}{\lambda_1} - \frac{1}{\lambda_2}$. This law can be true only approximately unless the wave-lengths are reduced; for it cannot depend on the temperature and pressure of the air. Although the approximation is great on account of the index of refraction having nearly the same value for all wave-lengths considered, there are cases where the difference is appreciable, as for the widely separated pairs of lines in the spectrum of Thallium.

As a further instance of the necessity of reducing wave-lengths to vacuo one may mention, that if the accuracy in the determination of wave-lengths were extended to less than one-hundredth of an Angström unit one would have to take account of the dispersion of air even in the method of coincidences. For a change of say four per cent in the density of the air might separate perceptibly two wave-lengths in the spectra of the first and third order that coincided before.

The dispersion of air has been measured by Ketteler, by Mascart and by Lorentz. Ketteler has determined the indices of refraction for three colours: Lithium 6708, Sodium 5893, and Thallium 5351, and Lorentz has repeated his measurements for the first two colors. Mascart has chosen four rays of Cadmium 6439, 5379, 5086, 4800 besides Sodium 5893. These are, as far as I know, the only determinations of dispersion. There exist besides a number of determinations for white light and several for Sodium light. But for wave-lengths shorter than 4800 no measurements of the index of refraction seem to have been made.

Professor Kayser and I have thought it necessary to measure the dispersion as far as we could photograph the wave-lengths. We have recently completed this work, and the full description of our method and observations is shortly to be published by the Berlin Academy of Science. The results may interest the readers of ASTRONOMY AND ASTRO-PHYSICS.

The dispersion was measured by letting the rays reflected from a large concave grating of Rowland pass through a prism of compressed air and measuring the deviation. Thus we found for dry air:

Wave-length.	Index of refraction for 0° and 760mm.	Weight.	Wave-length.	Index of refraction for 0° and 760mm.	Weight
5630	1.0002927	2	2860	1.0003088	1
4430	1.0002955	3	2850	1.0003094	2
4200	1.0002967	3	2550	1.0003158	2
3250	1.0003035	3	2360	1.0003219	1

These values agree very well with Cauchy's formula,

$$n = a + b\lambda^{-2} + c\lambda^{-4},$$

where n is the index of refraction, λ the wave-length and a, b, c positive constants. By the method of least squares we have calculated the formula $n = 1.00028817 + 1.316\ \lambda^{-2} + 31600\ \lambda^{-4}$ where λ is expressed in millionths of a millimetre. The greatest difference between the observed and calculated values amounts to 2 units of the last decimal. This formula allows to find the indices of refraction for other wave-lengths besides those of the observations. All measurements of the refraction of air for Sodium light, that have come to our knowledge are arranged chronologically in the following table:

Ketteler	1.0002947 in 1865
Mascart	1.0002927 in 1877
Lorentz	1.0002911 in 1880
Benoît	1.0002923 in 1888
Chappuis and Rivière	1.0002919 in 1888
Kayser and Runge	1.0002922 in 1893

As to the differences of indices for different colors our observations agree perfectly well with Ketteler's and Lorentz' observations and fairly well with those of Mascart. But the part of the spectrum that the measurements of these physicists comprise is far too small to allow an extrapolation for the whole. This is most evident, if a curve is platted whose ordinates are proportional to n and whose abscissæ are proportional to λ^{-2}. The curve will be nearly a straight line only slightly curved to a parabola. Now the former observers have measured this curve between $10^8\ \lambda^{-2} = 222$ and $10^8\ \lambda^{-2} = 434$, while our determinations allow to follow it to $10^8\ \lambda^{-2} = 1796$, that is, more than seven times as far. From our results the following table of corrections is calculated for reducing to vacuo Rowland's standard wave-lengths and all wave-lengths that are determined from them by interpolation.

Wave-lengths.	For reduction to vacuo add:	Wave-lengths.	For reduction to vacuo add:
8000	2.164 Ångström	4000	1.109 Ångström
7000	1.898 "	3000	0.857 "
6000	1.633 "	2500	0.739 "
5000	1.369 "	2000	0.635 "

For intermediate values of λ a linear interpolation gives the values exact within 0.002. These numbers we believe to be correct almost to unity of the last decimal.

There is a similar table of corrections given in the introduction to Watts' Index of Spectra, which is calculated from Ketteler's determinations of the refraction of air. The discrepancy between that table and ours is considerable, which is in a small degree due to the difference between Ketteler's and our observations and the wider range of the latter, but principally to the circumstance that Watts has not taken account of the temperature. He applies Ketteler's indices of refraction, which correspond to 0° Celsius to reduce wave-lengths that are measured at 16° Celsius. He ought, as we have done, to have computed the indices of refraction that refer to the same temperature to which the wave-lengths correspond and from them to have deduced the corrections.

The fact mentioned above, that one might have to take account of the dispersion of air in determining wave-lengths by the method of coincidences is clearly seen from the table of corrections. Suppose at a temperature of 20° Celsius and a pressure of 760 mm. two rays have the wave-lengths 6000 and 2000. They would then exactly coincide, if one was observed in the first the other in the third order. Now reduce both rays to vacuo. They would no longer coincide, the ray in the third order lying to the less refrangible side by $0.635 \times 3 - 1.633 = 0.272$. Since the corrections are proportional to the density of the air a barometric pressure of say 770 mm. and a temperature of 12° Celsius would make the distance between the two lines 0.011, an amount which is larger than the probable error that may be reached with well defined lines.

HANOVER, Germany, March 27, 1893.

ON THE VARIABLE STAR ALGOL.

BY THE LATE PROFESSOR WILLIAM FERREL.*

It has long been observed that many of the fixed stars vary in brightness, their maxima and minima recurring at regular periods, which vary in different stars from a few days to many years, and the variation of their brightness amounting, in some

* From "The Nashville Journal of Medicine and Surgery," April 1855, vol. VIII, pp. 277-282. Professor Cleveland Abbe has been kind enough to furnish a MS copy of this paper, which we reprint for its historical interest in connection with the recent investigations of Vogel and Chandler.

cases, to a total extinction of their light. The most remarkable and interesting of these is the star Algol, in the constellation of Perseus, both on account of the shortness of its period and the rapidity with which it changes from its ordinary brightness to its minimum, and back again. It was first discovered to be a variable star by Goodricke, in the year 1782, and it was observed that it remained at its ordinary brightness of a star of the second magnitude for about two days and 14 hours, when it rapidly changed in about 3½ hours to a star of the fourth magnitude, and then back again in about the same time to its ordinary brightness, completing a period in 2 days, 20 hours, 48m., 59s. So great was the regularity of its period at first, that 36 years after Goodricke discovered it to be a variable star, all the observations could be represented by a uniform period; but it has been shown by modern observations that its period is diminishing. Professor Argelander of Bonn, has been interested in this subject for several years, and although the number of observations of minima, especially in the first part of the present century, has been very few, so much so that for the first 40 years of the present century he only knew of 19 observed minima, yet from a discussion of all the observations made by himself and others, he has shown very conclusively, first that the period is diminishing, and secondly, that the diminution is not in proportion to the time. By treating the observations by the method of the least squares, he has arrived at the results given in the following table:

Periods of Algol.

Date	Length of Period.				
	d	h	m	s	s
1784, May 27	2	20	48	59.42	± 0.32
1788, Dec. 21				58.74	± 0.09
1793, July 11				58.39	± 0.18
1805, Nov. 25				58.45	± 0.04
1818, April 13				58.19	± 0.10
1830, July 3				57.97	± 0.05
1842, Sept. 20				55.18	± 0.35
1848, July 18				53.37	± 0.98

It is evident from the nature of the observations that no one observation can give anything like a reliable result. In order, therefore, to arrive at a knowledge of the law by which the period varies it will be necessary to have a great many observations, taken by different observers, at different times and places, so that the mean of all may be taken, and so that by discussing different sets of observations we may know what reliance may be placed on the accuracy of the results. Professor Argelander is therefore desirous that the number of observers should be greatly

increased, and he has, accordingly, published an article in the *Astronomical Journal*, Cambridge, Mass., in which he earnestly entreats American astronomers to give their attention to the subject. In order to facilitate the observations, he has computed the approximate times of all the minima visible in America during the present year. Those belonging to the remaining part of the year (1855) are given in the following table:

Minima of Algol, Visible in America, Washington Mean Time.

	h	m		h	m		h	m
April 12	9	16	Sept. 2	17	49	Nov. 10	13	18
May 19	15	52	5	14	38	13	10	7
June 11	14	22	8	11	27	16	6	56
July 1	16	3	11	8	16	27	18	11
" 4	12	52	25	16	19	30	15	0
" 7	9	41	28	13	7	Dec. 3	11	49
" 24	14	31	Oct. 1	9	56	6	8	38
" 27	11	20	4	6	45	9	5	27
" 30	8	9	15	17	59	20	16	43
Aug. 13	16	11	18	14	48	23	13	32
" 16	12	59	21	11	36	26	10	21
" 19	9	48	24	8	25	29	7	10
" 22	6	37	Nov. 7	16	29			

I have noticed this subject in a Southern journal, at the suggestion of Dr. B. A. Gould, Jr., of the *Astronomical Journal*, in a private letter which I have had the honor to receive, in the hope that there may be some amateurs of the science in the South who will be willing to respond to the call of Professor Argelander, and give their attention to this interesting subject. Any one can make the observations, as no instruments are required, and it is not necessary to secure the time with a very great degree of accuracy. Dr. Gould very kindly offers to furnish any one who may be disposed to make the observations, with a copy of the number of the *Astronomical Journal* containing Professor Argelander's article, in which directions are given for making the observations.

I would here suggest that the diminution of the length of the periods may be owing to a variable diminution of the distance between us and Algol, just as the periods of the satellites of any of the planets would appear to vary annually if no allowance were made for the equation of light. But it is evident that the diminution of the distance would only produce a change in the length of the period without affecting its uniformity. It is also evident that the variation of distance which would account, upon our hypothesis, for the diminution of the period, cannot result from a change of distance on account of the proper motions of the Sun and of Algol around some great central Sun of the uni-

verse, as Mædler's; for such motions, being connected with great revolutions and immense periods of time, would produce an apparent diminution in the period, which would not differ sensibly, in a few centuries, from a uniform diminution, which is entirely at variance with the observations which have been made. A variable diminution of the distance, then, between the solar system and Algol, such as would account, upon our hypothesis, for the phenomenon in question, can only arise from some revolution of the Sun or of Algol, connected with a comparatively short period of time, as a few centuries. But we know that the Sun has no such motion, and therefore, if there is any such motion it must be connected with Algol. But here the question naturally arises, where is the centre of attraction around which Algol revolves? I answer some large opaque body in its vicinity. Now, this may seem like very unphilosophically inventing a very improbable hypothesis in order to account for a particular phenomenon; but it is precisely the conclusion to which Bessel came from other considerations, with regard to Sirius and Procyon. From a discussion of the most accurate observations which have been made upon these stars, he ascertained that their motions deviate sensibly from uniformity, and hence he inferred that they must each revolve around some non-luminous body in their vicinity, and consequently that they are double stars, one of the members of which is non-luminous. That Algol may be a star of this kind, and that there are many opaque bodies in space of the size of our Sun, as Bessel supposed, I do not consider very improbable, since it is well known that, even within the last century, many stars have disappeared from the heavens and consequently, unless they have been annihilated, which is improbable, they must now exist as opaque and invisible bodies in space. And if we suppose the same thing to have happened for innumerable ages before, there must now be very many such bodies in space, and hence there may be many such double stars as Bessel supposed Sirius and Procyon to be. In fact, suns or stars, like almost everything else, seem subject to mutations, and after going through a series of physical changes, or perhaps cooling down to a certain temperature by the radiation of heat, seem suddenly to cease to shine; and our own Sun, which has been known to have dark spots on its surface 50,000 miles in diameter, may now be in the last stages of incandescence, and ere long cease to shine, and be as a missing star in the firmament of other systems of worlds. But it matters not, so far as our first hypothesis is concerned, whether there are opaque bodies in space or not, to produce a variable

motion in the stars, provided it is an established fact, that they do have this kind of motion, and that this motion in Algol is adequate to account, upon our hypothesis, for the observed diminution of the period.

Let us now endeavor to determine whether the hypothesis of such a motion in Algol is consistent with what we know of its motion, or of the motions of stars in general, based upon observations of their proper motions, and calculations of their distance. Without going into an accurate investigation of the subject, by means of mathematical formulæ, it may be easily determined approximately, from the table of diminishing periods which has been given, that the minima of 1848 occurred about 16000 seconds earlier than they would have done had the length of the periods not been diminished any since the year 1784. This number multiplied by 190,000 miles, the distance through which light passes in a second, gives in round numbers about 3,000 millions of miles for the space through which the light would pass in that time. Hence, in order to account for the minima's occurring 16000 seconds earlier and for the consequent gradual diminution of the periods, it is only necessary to suppose that Algol, in 1848, was 3000 millions of miles nearer the solar system than it would have been had it moved on with the same uniform velocity which it had in 1784. Great as this distance is, at the distance of Algol it could only be detected by the most accurate observations. For according to Struve, the average parallax of stars of the second magnitude is a little over $\frac{1}{10}$ of a second, a motion of 3000 millions of miles in an arc at Algol would only subtend an angle of about 3 seconds. If we suppose the variation of distance to be produced by the motion of Algol in an orbit, it is evident, that about the year 1784, and since, its motion must have been for the most part in a direction from the solar system, and consequently it would not have affected its apparent proper motion to the amount of 3 seconds. Now a variation of 3 seconds from uniformity in the proper motions of the stars since the year 1784, could not have been detected unless by the most accurate observations. Hence, if even a variable motion in any of the stars had never been detected, our hypothesis is not at variance with anything we know about the motions of the stars, and is in some measure confirmed by the discovery of Bessel. An orbit with a period of several centuries might be assigned to Algol, upon our hypothesis, with such eccentricity and position, that the equation of light, applied to the observations which have been made, would render the periods very

nearly equal, and in the course of time, the orbit might be pretty accurately determined from the observations of the minima; and then this, together with the observations upon the variable proper motion of Algol, if such motion should ever be detected, might furnish much more reliable data by which to calculate the *distance* of Algol, than any which we now have. It is considerations of this kind which invest this subject with a very high degree of interest.

NOTE ON THE SPECTRA OF THE FLAMES OF SOME METALLIC COMPOUNDS.*

G. D. LIVEING AND J. DEWAR.

A study of the spectra of flames offers many points of interest. It is long since A. Mitscherlich (*Poggendorff's Annalen*, vol. 116, p. 499; vol. 121, p. 459) showed that the spectra of flames are, for the most part, those of compounds of the elements present, and contain comparatively few rays proceeding directly from the elements themselves. But there are many questions still undecided. For example, it is not known whether the vibrations which give the spectra of compounds in flames are those which the molecules of the compounds in question would assume under the action of a high temperature alone, or whether they are not vibrations of a different order, arising during chemical changes, and deriving their energy directly from the chemical energy of the interacting substances. When the absorption spectrum of a compound is observed to correspond with its emission spectrum in a flame, we may infer that the vibrations are those which the compound molecule assumes when sufficiently heated. But there are not many cases in which this has been observed. We have observed it in the case of cyanogen (*Roy. Soc. Proc.*, vol. 44, p. 247, note), but we are not certain of any other case. The difference between the spectrum of the base of a flame and that of the upper part, observed in many flames, lends support to the supposition that there are rays which originate in the chemical change, perhaps occurring in the molecules which are in intermediate stages of the change, and not assumed by the molecules which are the final product, even when intensely heated in the upper part of the flame. The fact that the same rays which are seen in the base of a flame may be sometimes generated by electric discharges in the gases

* *Proc. Roy. Soc.*, Vol. LII. pp. 117–123.

which are burnt in the flame, or in their products of combustion, is not at all inconsistent with this supposition, for such discharges certainly have electrolytic effects, and may very well give rise to molecules in the intermediate stages between one state of chemical combination and another.

It is sometimes assumed in books on chemistry that the atoms which form a chemical compound can never be in an intermediate state between complete separation and complete combination. So inconceivable an assumption would hardly have been made except to support a theory, but it has nevertheless obtained a certain currency. It is supported by no fact and no analogy. Two atoms which are within the spheres of each other's influence, but have not yet reached the state of relative tranquility which we recognize as chemical combination, may very conceivably be the seat of very violent agitation and vibratory motions, which cease when they are actually combined. The flames of substances, such as the organo-metallic compounds, into which metals enter as chemical ingredients, have not hitherto, so far as we know, been observed, and it is to two such flames that these notes refer.

Spectrum of the Flame of Nickel-Carbonyl.

The remarkable compound of nickel and carbonic oxide, $Ni(CO)_4$, discovered by Mr. Mond, burns in air with a luminous, smoky flame, and the spectrum it emits appears to be a continuous one. When the vapor is burnt in oxygen instead of in atmospheric air, the spectrum still appears to be quite continuous; in fact, such a spectrum as carbonic oxide, without any nickel, gives under similar circumstances. This, however, is only in appearance, because the brightness of the continuous spectrum overpowers the feebler bands and lines which belong to the flame of the nickel compound. These bands and lines come out when the vapor of the nickel compound is diluted with a good deal of hydrogen. We have employed two methods of making such a mixture. The first was by passing a stream of dry hydrogen, mixed with carbonic oxide, over reduced nickel in a glass tube, and burning the issuing gas in a double jet with oxygen either outside or inside the burning gas. The nickel was freshly reduced at a gentle heat with hydrogen, and allowed to cool in carbonic oxide. When quite cold the stream of mixed hydrogen and carbonic oxide was found to take up quite enough nickel at the temperature of the room, and would continue to do so for some hours. After a time, however, the nickel required to be again

warmed in a current of hydrogen, when some water was given off, and the metal recovered its sensitiveness. Another plan was to pass a stream of hydrogen through a U-shaped tube containing a little of the liquid nickel compound in the bend. The result was the same in each case, but the proportion of vapor of the compound was more easily varied (by simply varying the proportion of carbonic oxide in the stream of gas) in the former method. The mixed gas and vapor burnt in air with a smoky flame, but in a full supply of oxygen with a bright yellowish-green flame without visible smoke. The first jet we used was of platinum, but nickel-carbonyl deposits nickel at a red heat, so that the platinum soon became coated with a thick deposit of nickel, which choked the orifices. This nickel adhered so closely to the platinum that it could not well be removed mechanically, and had to be dissolved off. We found it therefore more convenient to use a jet made of a piece of porcelain tube, about 1 cm. in diameter, with a narrow porcelain tube, fitted by means of a cork, in the axis of the wider tube. The mixed gas and vapor were passed either through the inner or through the outer tube, and oxygen through the other. The porcelain being a bad conductor, no nickel was deposited on it, except close to the orifice, whence it could be easily removed mechanically without disturbing the apparatus. The porcelain, of course, added some lines to the spectrum, but these were easily detected. In fact, we noticed only the lines of sodium, calcium and lithium.

The spectrum of the flame of the nickel-carbonyl thus diluted consists of two parts: (1) the spectrum of the main body of the green flame, (2) that of the base of the flame when the oxygen is outside, and of the surface of the small inner cone when the oxygen is inside, the flame.

The spectrum of the main body of the flame consists of a series of shaded bands, brightest in the green, but extending on the red side beyond the red line of lithium, and on the violet side well into the blue, though with rapidly diminishing distinctness. These bands have their sharp bright edges on the more refrangible side, that is, they are turned in the opposite direction to the bands produced by electric discharges in carbonic oxide at low pressure. The positions of the bright edges of the bands in the flame of the nickel-carbonyl have some correspondence with those of the bands produced by electric discharges in carbonic oxide, but it is not a very close one, and may be only accidental. With the dispersion employed, which gives a difference of deviation of $2° 52'$ between D and F, there was no sign of a resolution of these bands into

lines. The brightest bands had, however, their more refrangible edges pretty sharply defined, while the less bright bands, especially those in the blue, were very hazy. A certain amount of continuous spectrum, of course, overlay the bands, and made them somewhat less distinct. Photographs show that this continuous spectrum continues as far as λ 3500, but fading sensibly from λ 4200 onwards. The photographs do not show any extension of the bands beyond the blue.

Besides the bands a few lines, but only a few, in the visible part of the spectrum, extend into the upper part of the flame. Of these few only one is a known line of nickel; it is the green line λ 5476. This was also the only line of nickel which we observed in the visible part of the spectrum in explosions of hydrogen and oxygen in a nickel-lined tube (*Roy. Soc. Proc.*, vol. 36, p. 475).

In the ultra-violet part of the spectrum of the flame a great number of nickel lines were photographed; indeed, by far the greater part of the lines of nickel found by us in the arc (*Phil. Trans.*, vol. 179 (1888) A, p. 247), are between λ 3972 and λ 2943.5. In this case also there is a close correspondence between the spectra of the flame and of the explosions, except that the lines of the flame are much more numerous than those recorded of the explosions. This difference, however, is probably due to the much shorter exposure of the photographs of explosions. Although the photographs show lines as high as λ 2943, the lines in this region are very faint, and gradually die out in proceeding from the less to the more refrangible side of the spectrum. In the region about L, M and N the lines are very strong, so that it is for rays of those rates of vibration that nickel is most sensitive at the temperature of the flame.

Turning now to the base of the flame, we find a great number of lines, of which most extend but a short distance from the bottom of the flame. They form two principal groups, one in the orange and red, and the other in the citron and yellow. These lines are for the most part sharply defined, and in the more refrangible parts of each group very fine and closely set. They are probably channellings following Rydberg's law, and somewhat confused by overlapping.

None of these lines appear to be nickel lines, and, as they are limited to the base of the flame, they cannot be ascribed to any of the final products of the combustion, such as nickel oxide, but must be due either to the as yet unaltered molecules of nickel carbonyl, or to some molecules intermediate between that compound and the products of combustion which have only a transitory ex-

istence, and may, perhaps, have a transitory agitation of a particular kind imparted to them by the chemical energy which changes its form in the combustion.

The following table gives the approximate oscillation frequencies of the edges of the principal shaded bands, and of the lines seen at the base of the flame, but the numbers are only approximate:

OSCILLATION FREQUENCIES OF EDGES OF SHADED BANDS.

1496	1692	1933	2146
1521	1752	1960	2172
1577	1808	2052	2199
1594	1849	2107	2226
1635			

OSCILLATION FREQUENCIES OF LINES IN THE BASE OF THE FLAME.

1497	1582	1622	1721
1506	group of very	1627*l*	1727*l*
1509	closely set lines	1631*l*	1732*l*
1514	1586	1651	1735
1518	1593	1656	1738
1521	1595	1663	1741*l*
1526	1596	1667	1742
1543	1598	1671	1745
1547	1599	1673	1746
1549	1600	1682	1747
1555	1602	1686	1753
1560	1604	1690*l*	1806
1563	1607	1706	1809
1572	1612	1712	1827
1575	1615	1714	1833
1578	1616	1716	1879
1580	1618		

The six numbers in the above table to which an *l* is added correspond to lines which extend into the upper part of the flame.

TABLE OF WAVE-LENGTHS OF NICKEL LINES PHOTOGRAPHED FROM THE FLAME.

2943.5	3053.9	3194.9	3285.0	3409.0	3483.1	3587.2	3737.0
2981.2	3057.2	3196.6	3315.1	3413.4	3485.2	3601.4	3745.0
2983.6	3064.2	3201.5	3319.7	3413.8	3492.3	3609.8	3775.0
2992.2	3080.3?*	3221.1	3321.6	3423.1	3500.0	3612.1	3783.0
2994.1	3096.6	3224.6	3361.0	3433.0	3509.7	3618.8	3791.0
3002.1	3098.6	3226.3	3365.5	3436.7	3514.4	3624.1	3806.6
3003.2	3101.1|	3232.6	3367.2	3445.7	3519.1	3663.4	3831.7
3011.5	3101.4|	3234.2	3368.9	3452.3	3523.9	3669.7	3857.8
3018.8	3105.0	3242.6	3371.3	3457.8	3527.1	3673.4	3972.0
3031.4	3113.7	3247.8	3373.6	3461.1	3547.5	3687.6	
3037.5	3133.6	3250.1	3380.0	3466.8	3561.1	3694.6	
3044.5	3145.5	3270.6	3390.4	3468.9	3565.7	3721.6	
3050.4	3183.8	3282.2	3392.4	3470.8	3571.2	3736.1	

* A query is placed against this number because the water spectrum is so strong at this point that we cannot certainly distinguish the nickel line. There is no other reason for doubting its presence.

The more refrangible lines in the foregoing table were very faintly depicted on the photographic plate, and it is possible that a more lengthened exposure than the fifteen minutes, which we employed in the region where the lines were faint, would have brought out more lines. The continuous spectrum of the lime-light extends some distance further than the most refrangible of these nickel lines.

Flame of Zinc Ethide.

Zinc ethide burning undiluted produces so much continuous spectrum as to overpower any special rays. But by passing a stream of hydrogen through a bent tube containing zinc ethide, and burning the mixed gas and vapor in oxygen, as we did the nickel-carbonyl, we reduced the continuous spectrum sufficiently to enable us to observe any fairly strong rays which might be peculiar to the flame. In the visible part of the spectrum the three well-known rays of zinc in the blue λ 4812, 4721, and 4681 were easily seen. Photographs of the more refrangible part of the spectrum showed no trace of the ultra-violet lines of zinc; no more, in fact, than the flames of hydro-carbons usually show. Nor did there appear to be any rays from the base of the flame other than those seen in hydrocarbon flames in general. In our observations on explosions (*loc. cit.*) we did not find that a zinc lining to the tube in which the oxyhydrogen gas was exploded brought out any zinc lines, either in the visible or the ultra-violet part of the spectrum. The flame of the compound containing zinc chemically combined may be supposed to give the rays of zinc more readily than the exploding gases, which merely take up the metal mechanically. But the flame does not, in either case, seem hot enough to develop the ultra-violet rays, though these are very strongly developed in the arc.

ON A CERTAIN ASYMMETRY IN PROFESSOR ROWLAND'S CONCAVE GRATINGS.*

I. R. RYDBERG.

I. In order more especially to obtain a series of observations fitted for a continuation of the studies on the spectra of the éléments of which the commencement has been published in my "Recherches sur la constitution des spectres linéaires des èlè-

* *Philosophical Magazine* No. 214.

ments, chimiques" (*K. Svenska Vetensk. Akad. Handl.* Bd. XXIII, No. 11), a spectroscope with one of Professor Rowland's concave gratings (10,000 lines to the inch) was procured for the Physical Institution of the University of Lund. It was mounted in a most excellent manner by the Mechanician of the Physiological Institution, Hilding Sandström, according to the instructions of Professor Rowland (*see* Ames, Johns Hopkins University Circulars VIII, No. 73, May 1889; ASTRONOMY AND ASTRO-PHYSICS, January 1892 but with full freedom in the details of construction. The adjustments also were executed according to the same instructions, but with greater precision in the special determinations with the intention to obtain by the exactness of the adjustment the same scale through the whole spectrum.

However, when all adjustments were completed, no distinct image could be obtained in any part of the spectrum. In the visible spectrum of the first order the image was not very much out of focus, but the deviation increased gradually, so that it became necessary to displace the eyepiece several centimeters to obtain well-defined images of the spectra of higher orders. All details being executed with the same accuracy, there was nothing that could indicate the cause of the discrepancy, so that nothing remained but to make all the adjustments over again, determining at the same time the extreme limits of the errors. For this purpose I have made use of new methods of adjustment, and I have ascertained by these researches:

1. That the courses which the apex of the grating and the cross hairs of the eyepiece follow in their movement on the rails do not deviate in any point from straight lines by more than 0.2 millim.

2. That the angle formed by the average directions of the rails did not differ from a right angle by more than 15″ (corresponding to an arc of 0.5 millim. at one of the ends of one of the rails), the difference probably not amounting to more than a third of this value.

3. That the middle of the slit could not be more than 0.2 millim. from the crossing-point of the lines that are described by the apex of the grating and the cross hairs of the eyepiece.

4. That the apex of the grating and the cross hairs of the eyepiece were not more than 0.1 millim. distant from the axes of the carriages.

5. That the distance between the centre of curvature of the grating and the axis of the carriage on which the eyepiece was placed, did not amount to 0.5 millim. during the whole movement.

XUM

6. That the lines of the grating and the direction of the slit were parallel and at right angles with the plane of the rails.

7. That the grating was entirely free from all constraint and of spherical form, the images in the centre of curvature being of excellent definition.

8. That the optical state of the slit was perfectly normal.

With these results it was only in the grating itself that the cause of the displacement of the spectra could be looked for, either in some imperfection of the theory or in some fault in the execution of the work, at least with regard to the special grating in question. Hitherto, I had not deemed it possible to make any of these assumptions, as it seemed that Professor Rowland himself and other spectroscopists who have used the concave gratings ought to have recognized such an anomaly, if it existed.

During all the adjustments the grating was left in the same position in its holder, so that I had made use only of the spectra on one side of the grating. Now it was removed from its holder and, after being reversed, it was adjusted in the same manner as before, with the intention of learning whether the focal curve that passes through the centre of curvature is symmetrical with respect to the principal axis of the concave mirror. Then it was found that the distance between the grating and the eye-piece ought to be increased in order to get distinct images, while before it was necessary to diminish it. From this it was evident that the inaccuracy in the position of the images was due to the grating.

II. First of all the question was to determine the true form of the focal curve that passes through the centre of curvature of the grating. According to Professor Rowland's theory this ought to be a circle, which should have as a diameter the straight line that unites the centre of curvature with the apex of the grating. If the form of the curve differed perceptibly from a circle, it would not be possible with these gratings to obtain spectra of a uniform scale.

The form of the focal curve can be determined with the greatest facility, if the apparatus is altered in such a manner that the slit is made movable along the rail that carries the grating.

Let G_1G_2 (Fig. 1) be the grating, C its apex, O its centre of curvature, CLOM the theoretical focal circle, $CL_1O_1OM_1$ the true focal curve that passes through O, L the slit in its original place at the point of the right angle which is formed by the rails LC and LO. Then, on displacing the slit along LC or its elongation to a certain point L_1, it will always be possible to obtain distinct images

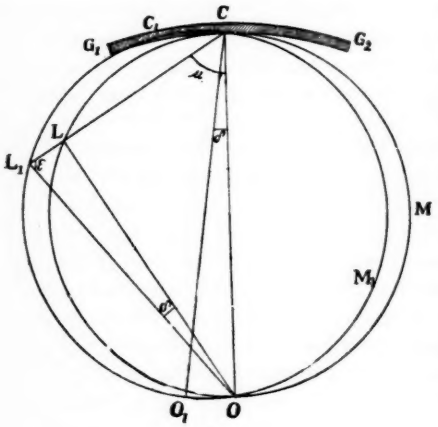


FIG. 1.

of the spectra, supposing in all cases that such can be given by the grating. This point L_1 belongs to the curve in question, whose polar coordinates will be determined by measuring the radius of curvature CO (ρ), the displacement LL_1 (d), and the angle LCO (μ). For then, supposing d to be positive, when the slit is moved away from the mirror, the radius vector $L_1C = r = \rho\cos\mu + d$ and the vectorial angle $= \mu$.

Denoting the wave-length by λ, the number of order of the spectrum by n, the distance between two adjacent lines of the grating by ω, we should have, if the theory were exact,

$$\sin\mu = \frac{LO}{\rho} = \frac{n\lambda}{\omega}, \quad n\lambda = \frac{\omega}{\rho}.LO.$$

To decide whether the same formula can be applied in the present case, it will be sufficient to displace the slit along the rail LC and to observe if any change is produced in the spectrum, viz., if the same value of μ always corresponds to the same value of λ independently of the value of r. In reality small irregular variations were found, which did not seem to exceed one of Angström's units, and which were doubtless due to imperfections in the rails and the adjustment. According to the theory of concave gratings a difference of one Angström's unit in the spectrum corresponds to a lateral displacement of the slit varying in the spectra of different orders between 0.25 and 1 millim. Consequently the formula is exact within the limits of error of our experiments. It follows that the angle ε of the segment CL_1O is determined by the equation

$$\cot\varepsilon = \frac{d}{LO} = \frac{d\omega}{\rho \,.\, n\lambda}.$$

In this formula d and $n\lambda$ being the only variables, we see that it is sufficient that their quotient $\frac{d}{n\lambda}$ be constant, in order that the angle ε may be so too.

III. The measurements were executed in such a way that the spectroscope was directed on some known line of the spectrum and the movable carriage that bears the slit displaced along the rail LC, until a distinct image was obtained in O. The position of an index, attached to the carriage, was read on a millimetre-scale fixed to the rail LC. Each of the numbers given in the following table under a_1 and a_2 is the mean of 10 of these readings, the carriage being alternately brought near to and removed from the grating. The column a_1 corresponds to the spectra on one side of the grating, the column a_2 to those on the other; a greater value denotes a greater distance from the grating. It was found that the slit could be displaced through the space of about one millimetre without it being possible to distinguish any variation in the definition of the image. The probable errors of the means amount in general to 0.2 millim., they never exceed 0.4 millim. Using as a source of light sometimes the Sun, sometimes the voltaic arc, I directed the spectroscope in the spectra of the first four orders to the weak lines between D_1 and D_2 ($\lambda = 5893$) and to the double lines b_3 and b_4 of the solar spectrum or the strong doublets of the neighboring band of carbon (λ about 5165):

Spectral line.	Wave-length $10^{-7}\, n\lambda$	a_1	a_2	$\frac{1}{2}(a_1+a_2)$	Observed.		Calculated.
					d_1	d_2	d
I. b_4...	1×5165	133.8	153.8	143.8	− 10.0	+ 10.0	9.4
I. D...	1×5893	133 4	155.6	144.5	− 10.4	+ 11.8	10.8
II. b_4...	2×5165	123.2	161.7	142.5	− 20.6	+ 17.9	18.9
II. D...	2×5893	121.8	165.2	143.5	− 22.0	+ 21.4	21.5
III. b_4...	3×5165	115.8	171.6	143.7	− 28.0	+ 27.8	28.3
III. D...	3×5893	112.0	175.7	143.8	− 31.8	+ 31.9	37.7
IV. b_4...	4×5165	106.0	181.6	143.8	− 37.8	+ 37.8	32.3
IV. D...	4×5893	101.4	187.6	144.5	− 42.4	+ 43 8	43.0

Mean 143.8

The first column contains the number of order of the spectrum and the line to which the spectroscope was directed; the second the approximate wave-length. In the fifth column are the means of the values of a_1 and a_2, which correspond to the same line in the spectra on the two opposite sides of the grating. These means approach, as we see, to a constant value 143.8, which corresponds evidently to the normal position of the slit at the vertex of the right angle formed by the rails. On determining by direct measurements this position, I have found 144.8 ± 0.1. But the

difference of one millimetre between the two numbers is perfectly explained through the uncertainty in the two adjustments of the centre of curvature of the mirror on the axis of the carriage of the eye-piece, first in the direction of the girder that unites the two carriages, and secondly, in the lateral direction. Of this I have convinced myself by another series of determinations, by displacing intentionally the centre of curvature. A fault of 0.5 millim. in the determination of the radius of curvature is sufficient to explain the before-mentioned difference.

Thus the point 143.8 is to be considered as the vertex of the right angle of the rails, through which passes in the present case the theoretical focal circle of the grating, or rather a curve which differs from it very slightly. Using this number (a_0) I have calculated the differences $d_1 = a_1 - a_0$ and $d_2 = a_2 - a_0$, which are found in the table under the heading "observed." A glance at these numbers shows that they are at least very nearly proportional to the corresponding values of $n\lambda$, which implies that the angle ε of the segment of the true focal curve is constant. To examine this more closely, we will insert in the preceding equation of ε

$$\frac{\rho \cot \varepsilon}{\omega} = x,$$

and we will calculate by the method of least squares the exact value of x from the 16 equations of the form

$$x \,.\, n\lambda = d,$$

which we obtain from the preceding table by using all the values of d_1 and d_2.

In this way we find the value

$$x = \frac{\rho \cot \varepsilon}{\omega} = 18261 \pm 80.$$

The numerically equal values of d_1 and d_2, which are obtained on making use of this value of x, are given under d in the last column of the table. The differences between these numbers and the observed values being confined within the limits of errors of observation, it must be considered as proved that *the angle ε in the segment of the focal curve is a constant.*

A segment of which the angle is a constant belonging necessarily to a circle, we can express the result of our researches as follows:—

The focal curve which passes through the centre of curvature of the mirror is a circle, which, however, has not the radius of curvature in the apex of the grating as a diameter.

Always supposing, as in the preceding, that this curve also

passes through C, we see on fig. 1 where CO_1 is a diameter of the true focal circle, that the angle between CO and CO_1 is $=\delta=\frac{\pi}{2}-\varepsilon$. We have then

$$\tan\delta = \cot\varepsilon = 18261\,\frac{\omega}{\rho}.$$

The determination of the radius of curvature has given

$$\rho = 6434 \pm 1 \text{ millim.}$$

According to the statement engraved on the grating, $\omega = 0.0001$ inch $= 0.0254$ millim. From this we obtain $\delta = 24'\,47''$ and the arc $OO_1 = \rho \tan \delta = 18261\ \omega = 46.4$ millim. The difference between the diameters CO and CO_1 amounts to 0.17 millim.

IV. Though there could be no doubt as to the obliquity of the grating, it was possible that we had to do with some accidental anomaly peculiar to our special grating. For that reason it was of great interest for me to find an opportunity to examine another grating of the same kind, and this has been made possible through the kindness of Dr. A. E. Andersson at Kristianstad, who has been good enough to place at my disposal a concave Rowland grating of exactly the same kind as the preceding.

The measurements, which have been executed in the same order as before, follow here:—

Spectral line.	Wave-length $10^7\,n\lambda$	a_1	a_2	$\frac{1}{2}(a_1+a_2)$	Observed.		Calculated.
					d_1	d_2	d
I. b_4...	1×5165	130.3	155.6	143.0	−12.3	+13.0	11.6
I. D...	1×5893	129.2	158.5	143.9	−13.4	+15.9	13.2
II. b_4...	2×5165	118.3	164.6	141.5	−24.3	+22.0	23.2
II. D...	2×5893	115.5	168.7	142.1	−27.1	+26.1	26.4
III. b_4...	3×5165	108.6	177.4	143.0	−34.0	+34.8	34.8
III. D...	3×5893	104.2	181.1	142.7	−38.4	+38.5	39.7
IV. b_4...	4×5165	95.5	189.6	142.5	−47.1	+47.0	46.4
IV. D...	4×5893	89.7	195.3	142.5	−52.9	+52.7	52.9

Mean 142.6

The probable errors of the values of a are a little greater than in the preceding case, and amount in maximum to 0.5 millim. Moreover the adjustments of the grating are not of quite the same exactitude, owing to the limited time I had at my disposal for making these determinations. Hence there is nothing astonishing in the fact that the mean, 142.6, of the values of a differs a little more from the normal value, 144.8, than is the case with

the other grating. The error necessary to produce such a difference does not exceed $\frac{1}{6000}$ of the distance to be determined.

On calculating as before the quotients $\frac{d}{n\lambda}$, we obtain

$$\frac{\rho \cot \varepsilon}{\omega} = 22440 \pm 120.$$

The radius of curvature of this grating was found to be 40.3 millim. greater than that of the other, viz., 6474 millim., the uncertainty amounting to about 2 millim. With this value of ρ and the nominal value of ω we find $\delta = 30' 16''$ and $OO_1 = 57.0$ millim. To judge from the inscriptions on the gratings the obliquity ought to have the same direction in both cases.

V. The focal curve that passes through the centre of curvature of the grating being a circle, as shown above, it will always be possible to adjust the spectroscope in such a manner as to obtain in all positions distinct images of the spectra without altering the distance between the grating and the eyepiece. It follows from the preceding that this is effected by causing the girder to act the part of the diameter O_1C of the true focal circle instead of that of a radius of curvature of the grating. Hence in the first grating the girder ought to be lengthened by 0.17 millim., then turned round the point C through an angle $\delta = 24' 47''$. The easiest way of doing this is to displace the cross hairs of the eyepiece in O with the micrometer through a distance equal to the corresponding arc 46.4 millim. (to left or to right according to the side of the grating which we wish to make use of), and afterwards turning the grating until the cross hairs and their image coincide again. Then in all positions of the girder the slit ought to remain on the true focal circle. The exactitude of the theory and the measurements is confirmed by the fact that after the execution of these adjustments the image became perfectly defined through the whole spectrum.

It would also be possible to obtain distinct images in all positions by another method, viz., by altering the angle of the rails by a quantity δ. In that case the centre of curvature ought not to be displaced, but it is necessary to turn the micrometer and the photographic box through an angle δ, so that they may be always tangents of the focal circle.

However, the supposition which we have made, that the true focal circle passes through the apex C of the grating, does not possess any very high degree of probability, because in that case the curve would intersect the surface of the grating. It seems more likely that it touches it, but at another point, which, as can

be seen by construction and calculation, would be situated at a distance of 46.4 millim. from the apex C. In reality, the conclusion, that could be drawn from the measurements would remain almost the same, if we had replaced in fig. 1 the circle $CL_1O_1OM_1$ by a circle passing through O but not through C or O_1 and touching the grating at C_1. This circle would have a radius equal to ρ, that is to say 0.17 millim. less than that of the former. As to the variation $\Delta\delta$ of the angle δ, we find $\sin \Delta\delta = \frac{\sin^2\delta}{2\cos\mu}$, a quantity varying with μ, but which falls within the limits of errors of observation in all parts of the spectra, that we can use. The arc CC_1 differs from the arc OO_1 (46.4 millim.) only by some tenth of a millimetre. Under this new supposition an exact adjustment according to Professor Rowland's theory is obtained by displacing the grating 46.4 millim. along its own surface, until the point of contact of the focal circle falls in with the axis of the carriage, where the apex of the grating was situated before. The considerable obliquity that the position of the grating would show in that case is the only difficulty with this arrangement. Hence I have preferred the first method of adjustment, after having convinced myself that the difference is of no importance from a practical point of view.

VI. On the other hand, the last manner of considering the matter seems to possess a considerable advantage, because it will allow us to account in a simple way for the relations between the true focal circle and the grating. In reality, the accordance with Professor Rowland's theory is perfect and the obliquity is only due to the point of symmetry of the grating not coinciding with the apex of the concave mirror.

Let AC_1CBO (Fig. 2) be a section through the centre of curvature, perpendicular to the surface ACB and to the lines of the grating. Let AB be the chord of the section, which is perpendicular to the radius CO that passes through the apex of the spherical cap. The lines of the grating being drawn perpendicular to the axis of the dividing-machine (and perpendicular according to our supposition to the plane of the paper) it will always be possible to draw in the plane of the paper a straight line EF parallel to the axis in question. On a tangent plane TT_1 to the spherical surface, parallel to EF and perpendicular to the plane of the paper, the dividing-machine would draw equidistant lines. On both sides of the point of contact C_1 of this plane the distances of the corresponding lines are also equal, C_1 is the point of symmetry and the point of contact of the focal circle.

XUM

Then it is immediately seen by the figure that the angle which is formed by the straight lines AB and EF becomes equal to the angle δ between the radii CO and C_1O. AB being = 150 millim. the values of the differences between AE and BF in the two gratings are found to be 1.08 and 1.32 millim. respectively.

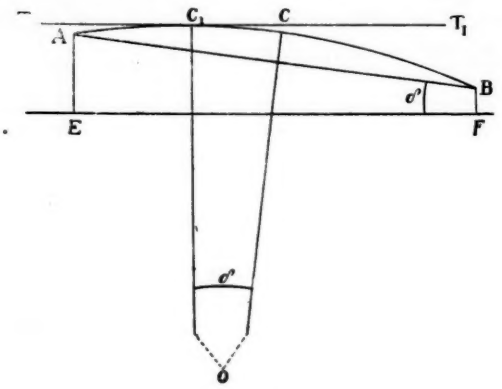


FIG. 2.

In this way the asymmetry of the gratings is explained in a manner as simple as it is complete. However, the differences found being much greater than would be supposed in a work of such perfection as are the gratings of Professor Rowland, we cannot exclude another hypothesis, the same that M. Cornu* has made use of in order to explain the focal properties of plane gratings, viz., a systematic variation in the distances of the lines. Such a variation would arise from an irregularity in the screw of the dividing machine, and in the plane gratings this explanation seems to be the only one possible. But in the concave gratings the same fault might also arise from another cause. The two sides of the gratings giving spectra of different brightness, we may conclude that the furrows which the point of the diamond makes in the reflecting surface are not symmetrical in section. Then it is easy to see that the distances of the lines are subject to a continual variation from one side of the grating to the other. But without knowing all the details of the work in the ruling of gratings, it would be useless to enter more closely into this hypothesis, and impossible to decide whether it is superior to the preceding.

* *C. R.* lxxx. p. 645.

THE MAGNETIC STORM AND AURORAS OF JAN. 7 TO 10, 1886.*

M. A. VEEDER.

This period of auroras and magnetic disturbance, which was the most prominent of any recorded during that month, affords the means of testing the question as to the location of the originating solar conditions in a very positive way. There was upon all the dates named a very extensive area of disturbance about ten degrees south of the Sun's equator and at the eastern limb appearing by rotation. Although much disturbed, as evinced by the brilliance and extent of the faculæ, no spots formed in this area until its advance portions were on the meridian on the 12th, when there suddenly appeared a large spot which increased rapidly in size, and was surrounded by numerous others upon the next two days, when the group became of enormous extent. Here, if ever, the conditions were favorable for any effect proceeding from the meridian, or just beyond, as Professor Ricco maintains in the January number of ASTRONOMY AND ASTRO-PHYSICS as being the possible rule. In spite, however, of the extraordinary character of this outbreak and its extremely rapid increase in the precise location whence the meridian effect ought to proceed, there was not the faintest increase of magnetic perturbations, a few very slight ones only having been in progress on the days preceding. Nor was there any report of an aurora of any kind. On the contrary on January 16th and 17th all magnetic disturbance ceased and absolutely smooth curves were traced for the only time during the entire month for so long a period. Contrast this behavior with the violence of the perturbations when the same area was at the eastern limb, and when it had not yet developed spots, and the difference is most strikingly manifest.

If this case stood alone it would be sufficient to discredit the theory that the inductive effect proceeds from the Sun's meridian. On the other hand its bearing upon the view that such effect proceeds from the eastern limb is equally plain, and is specially instructive because of its showing that not faculæ in general, but faculæ preceding the formation of spots, and recurring again and again in parts of the Sun frequented by spots are capable of exercising the auroral and magnetic effect. The writer had supposed that this had been made so plain in previous notes and articles that there could be no mistake. Inasmuch, however, as misapprehensions of one sort or another

* Communicated by the author.

XUM

are found to exist he is preparing to submit the evidence in detail that will show that cases such as that above described are not simply common, but follow an absolute rule, exceptions to which, if they exist at all, are very difficult to find.

LYONS, N. Y., April 14, 1893.

SPECTROSCOPIC NOTES FROM THE KENWOOD OBSERVATORY.*

GEORGE E. HALE.

THE SOLAR PROMINENCES AND FACULÆ OF APRIL 16, 1893.

The accompanying plate represents the prominences and faculae photographed at Chicago with the spectroheliograph at about the time of the total phase in South America of the solar eclipse.† The drawing was made from the original negatives by projecting on a screen of white paper with an optical lantern, and tracing the forms of the prominences and faculae. By this method it is possible to bring out faint details which would be lost in a direct photographic reproduction of the original negatives, at the same time preserving absolute truthfulness of form in the representation.‡

In spite of the fact that the sky was very white when the photographs were taken, both prominences and faculae are fairly well shown. As is the case in all "spectroheliograms," no details are visible in the spots, the smaller spots and the penumbrae of the larger ones being concealed by overlying faculae.§ The greatest solar activity is clearly in the southern hemisphere, where both faculae and prominences are more numerous than in northern. As this has prevailed for some time it would be natural to expect a corresponding development of the corona south of the equator. The long chain of prominences in the southwest quadrant is a noticeable feature of the photographs. It was photographed on April 15th and differed but little in form except by the absence of the two streamers which were of later origin. On April 17th this prominence had greatly decreased in size.

* Communicated by the author.

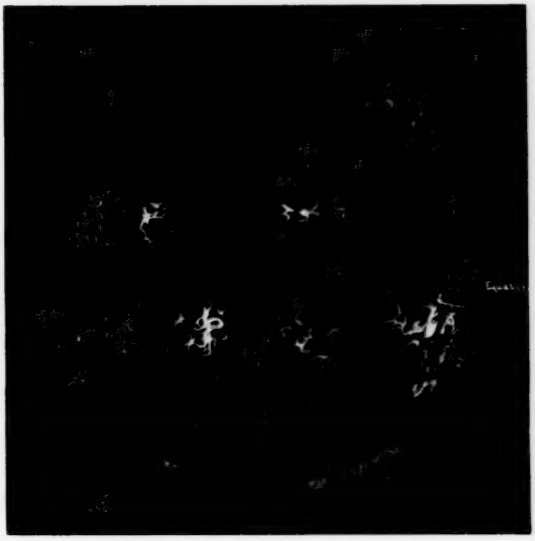
† The photographs used in making the illustration were four in number, two showing prominences and two faculæ. The former were taken at $9^h\ 15^m$ and $9^h\ 59^m$ A. M. and the latter at $9^h\ 0^m$ and $9^h\ 6^m$ A. M., Chicago M. T. Clouds prevented photographs being taken at the exact time of totality.

‡ The process of reproduction from the elliptical image of the original negatives was unfortunately not free from distortion. It will be noticed that the line representing the solar equator appears below the center of the image.

§ The positions of the spots on the figure are denoted by small crosses, but these will hardly be noticed without a magnifier.

XUM

PLATE XXV.



The Solar Prominences and Faculæ of April 16, 1893.

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XUM

It will be of interest to compare these results with those obtained during the eclipse. Some light may perhaps be thrown on the still mysterious "white prominences," and the complete record of the faculae may also be of value.

ON THE SIZE AND DISTRIBUTION OF SOLAR FACULÆ.

In recent photographic work with the spectroheliograph the second slit has been made very narrow and a piece of blue glass has been held over the first slit during the exposure. The advantage in photographing the faculae is very marked, as the greater part of the green region in the overlapping spectrum of the third order, which before fell on the photographic plate during the exposure, is now cut out. An examination of the absorption spectrum of the glass shows that the green is only partly absorbed, and with a better quality of glass it will no doubt be possible to still further improve the photographs.

I have elsewhere remarked on the great size and extent of the faculae as shown on photographs taken with the spectroheliograph. The negatives made with the blue glass absorbant, however, show that my estimates have been even too small. Faculæ hitherto invisible are now seen dotting the surface of the Sun from pole to pole. These faint faculæ do not appear to be separate and independent phenomena; over the whole surface of the Sun they seem to be connected like the meshes of a net, though the forms of the enclosed spaces are very irregular. Between latitudes + 40 and — 40 they are somewhat brighter than in the regions near the poles, and on the best photographs connecting groups can be traced for great distances. Some of these will be noticed on the accompanying plate, though the photographs which it represents were made under unfavorable atmospheric conditions, and do not bring out the faintest faculæ.

The greater part of these faint objects are quite invisible to the eye in telescopic observation, but they are probably identical with the small irregular reversals of the C line noticed when the slit of a solar spectroscope is moved across the Sun's image. Photographs taken here in December, 1891, showed that the reversals of the H and K lines were almost unbroken on the solar surface*, but hitherto the faintest of these regions have escaped the spectroheliograph. Though they seem to form a nearly continuous net-work they are of course not to be confused with M. Janssen's *reseau photospherique*.

* Father Sidgreaves has remarked such continuous reversals on photographs of spot spectra taken at Stonyhurst.—*Monthly Notices*, February, 1893.

Between these faint objects and the brightest faculæ, we find faculæ of every intermediate degree of brilliancy; sometimes a single photograph shows faculæ representing almost all the gradations of brightness. Groups of faculæ are at present (April 14) so numerous in the southern spot zone that they form an almost unbroken belt across the Sun parallel to the equator. One is led to ask whether such belts may not in the future become continuous—or are the indications those of a past condition of the Sun's life?

The curved forms of the faculæ, to which I have referred in previous papers, are frequently so marked that one can hardly doubt the evidence of cyclonic motion. The small size of the solar image in our present photographs makes an investigation of this subject somewhat difficult, but I hope to take it up soon with the 40-inch Yerkes telescope.

THE H AND K LINES IN THE CALCIUM SPECTRUM.

In the winter of 1890-91, while engaged in a study of the ultra-violet spectrum of burning magnesium ribbon, I detected on the photographs what appeared to be faint traces of the H and K lines of calcium. As these lines had never to my knowledge been obtained in the laboratory without electrical means, it was decided to investigate their seeming presence in the flame. To this end a large number of photographs were made, and the presence of the lines in the spectrum of burning magnesium ribbon, which had been passed through a solution of calcium chloride, was clearly established. In the fourth order spectrum of a concave grating (14,438 lines to the inch) of ten feet radius, the lines thus produced in the flame coincided absolutely with the H and K lines in the calcium spark. In the flame, however, the lines were so faint that they required an exposure of two hours, while a few minutes sufficed for the spark. The blue line at λ 4226.9, which is strong in the flame, is far less prominent in the spark.

The subject has recently been taken up at the Kenwood Observatory by Mr. S. B. Barrett, a graduate student in astro-physics at the University of Chicago. Since my laboratory work in 1890 much has been learned of the H and K lines in solar and stellar spectra, and Mr. Barrett has undertaken to continue the study in terrestrial sources of heat. He has confirmed the results obtained with the magnesium flame, and has also found both of the lines in the oxy-coal-gas flame. In the blast-flame and Bunsen burner they have not been certainly detected, though with the latter an exposure of over 100 hours was given, the flame being fed con-

tinuously by a wheel of platinum wire rotating by clockwork through a solution of calcium chloride. The solution is kept at a proper level in the tank by an automatic siphon. This little device of Mr. Barrett's is very useful for long exposures.

From the oxy-coal-gas flame to the prominences the H and K lines preserve the same relative intensity, K being invariably stronger than H. The following table is given to make evident the wide distribution of calcium, and the importance of exhaustively investigating its spectrum. The character of the lines was in all cases determined from photographs taken at the Kenwood Observatory.

Light Source.	*Character of H and K lines.*
Bunsen flame	Presence uncertain.
Blast flame	Presence uncertain.
Oxy-coal-gas flame	Faint, narrow, sharp.
Magnesium flame	Faint, narrow, sharp.
Electric arc	Strong, broad, hazy, reversed.
Electric spark	Strong, narrow, sharp.
Sun (general spectrum)	Very broad, hazy, dark bands; narrow dark line in center. Most prominent lines in spectrum.
Sun-spots	Bright, narrow, sharp in umbra; doubly reversed in penumbra. Strongest lines in spectrum.
Faculæ	Narrow, sharp, doubly reversed. Strongest lines in spectrum.
Chromosphere	Narrow, sharp, often doubly reversed; strongest lines in spectrum.
Prominences	Bright, narrower than in chromosphere, sharp, rarely doubly reversed; strongest and longest lines in spectrum.
α Lyræ	K dark, narrow; H masked by hydrogen ε line.

Many questions of interest from both physical and astronomical standpoints are involved in the investigation, and these will be discussed in a future paper.

A METHOD OF STUDYING THE DISTRIBUTION OF METALLIC VAPORS IN THE ELECTRIC ARC.

A careful study of the spectroheliograph has convinced me that this instrument, although primarily designed for photographing the solar prominences and faculæ, may have wider applications. To its possible employment for photographing the solar corona without an eclipse, I have referred in the last number of this journal. Experiments in this direction are now in progress at the Kenwood Observatory, but up to the present time the sky has shown so much whiteness from the city smoke, as well as from dust carried to great elevations by exceptionally high winds, that we have had to content ourselves with completing the adjustments of the apparatus, which is attached to the 12-inch equatorial. We can hardly hope under any circumstances to success-

fully photograph the corona in the heart of a smoky city, but I am fully convinced that some success would be attained if the method were tried from such an elevation as Pike's Peak. The method here referred to is that outlined in the last number of this journal—the reduction of the diffuse light of the sky near the Sun by setting the broad dark K band of the sky spectrum on the second slit of the spectroheliograph.

Still another use of the spectroheliograph—this time for laboratory purposes—has suggested itself. It is well known that the distribution of vapors in the electric arc is by no means uniform—different parts of the arc give different spectra. An excellent method of investigating the distribution would be to photograph the arc with a spectroheliograph, using successively different lines of the spectrum. The peculiarities of distribution about the positive and negative poles and in the flame, as well as the separation of different vapors, could thus be made manifest.

AN ERUPTION ON THE SUN'S SURFACE.

"Spectroheliograms" taken on Jan. 26, 1893, give a complete history of an eruption which occurred near the solar equator on the northern edge of a large group of spots and faculæ which a few days before had come into view at the eastern limb. The eruption was not unlike that photographed at the Kenwood Observatory on July 15, 1892, though it was less brilliant and on a smaller scale. The first indications of the outburst are seen in a spectroheliogram taken at $10^h\ 25^m$ (Chicago M. T.), and at $10^h\ 42^m$ the disturbance was at its maximum. At $11^h\ 17^m$ the mass of erupted matter had greatly decreased in brilliancy, and at $11^h\ 36^m$ it had disappeared. The eruption was not visible to the eye in an image of the Sun projected on a white surface by the eyepiece. I have not yet learned whether there was any accompanying magnetic disturbance.

ON THE HISTORY OF SOLAR PROMINENCE PHOTOGRAPHY.

In an article on the spectroheliograph in the March number of this journal I attributed to Dr. C. Braun, formerly director of the Haynald Observatory at Kalocsa, the credit of first pointing out the method upon which that instrument—itself devised independently in 1889—is based. This I now find to be a mistake. In the *Comptes rendus* for March 6, 1893, M. Janssen, after a very kind reference to the photographic results obtained at the Kenwood Observatory, shows that the principle of using a second slit and photographing the prominences by monochromatic light

was discussed by him at the Exeter meeting of the British Association in 1869. I had not previously seen the note referred to, and I now hasten to acknowledge my error, as well as my indebtedness to M. Janssen for the correction.

KENWOOD OBSERVATORY,
University of Chicago, April 17, 1893.

A PROPOSED METHOD OF DETERMINING WITH GREAT EXACTNESS THE INDEX OF REFRACTION AND THE DISPERSION OF AIR.*

B. HASSELBERG.

In the study of astronomical refraction as well as in various optical investigations a knowledge of the index of refraction of air for the different lines of the spectrum is well known to be of great importance. Numerous attempts to determine its value have been made since the beginning of the present century. In considering the different methods employed for this purpose one may divide them into two distinct classes, one of which, devised by Borda and employed for the first time by Arago and Biot† and later by Dulong‡, is based on the direct measure of the deviation of a luminous ray through a prism from which the air has been exhausted, while the other, devised by Arago§ and employed by Jamin‖ and Ketteler¶, is founded on the interference of two pencils of light, in one of which a suitable difference of path may be introduced. All of these investigations, with the exception of that of Ketteler, give only the mean index of refraction of air, but, as M. Mascart has recently shown, with a precision far from satisfactory.** The index of refraction obtained by M. Ketteler for the D line, having been determined by only two measures, does not seem to merit greater confidence than that which may be accorded to the values of Arago and Jamin, with which it is moreover almost identical. The indices of the other principal lines of the solar spectrum having been deduced from the first by an interpolation formula, are consequently subject to the same uncertainty as far as their absolute values are concerned; as for their

* *K. Vetenskaps-Akademiens Forhandlingar*, 1892.
† *Memoires de l' Institut*, T. viii, p. 301, 1806.
‡ *Annales de Chimie et de Physique*, 2 Sér., T. xxxi, p. 154.
§ *Ann. de Chim. et de Phys.*, 2 Sér., T. i., p. 1.
‖ *Ann. de Chim. et de Phys.*, 3 Sér., T. xlix, p. 282.
¶ *Poggendorff's Annalen*, Bd. cxxiv. s. 390.
** *Annales de l'ecole normale*, 2 Sér., T. vi, p. 9-78, 1877.

relative values the precision is certainly greater, though in no case of the high order which the great number of decimals employed would seem to indicate.

In this condition of things the question has recently been taken up again by M. Mascart* in a much more profound and rigorous manner. His method is also that of light interference, but for the end in view he profits by a special kind of interference —Talbot's bands. A complete theory of these singular phenomena was long ago given by Airy†, but this theory being extremely complex M. Mascart substitutes for it another of much greater simplicity. After having, moreover, carefully studied all the circumstances which can in any way affect the exactness of the results to be obtained, M. Mascart finally arrives at the following value for the index of refraction of air at zero temperature and 76 cm. pressure, for the D line of the solar spectrum:

$$n = 1.0002927.$$

The exactness of this value is estimated by M. Mascart as within about 6 units in the last decimal place. It will also be noticed that the refraction of air is sensibly less than is indicated by the researches of Biot and Arago, the final result of which is:

$$n = 1.0002945.$$

By these investigations of M. Mascart the question of the refraction and also the dispersion of air might be considered as definitely solved. This may be true for the greater part of the applications of these constants in astronomy, in spectroscopy, etc.; nevertheless an independent confirmation by a different method capable of great precision does not seem wholly without interest. For this reason I venture to propose the following mode of procedure:

Everyone is aware of the truly extraordinary precision which may be arrived at in the determination of the relative position of two spectral lines on a good photographic negative obtained by means of Professor Rowland's large diffraction gratings. This circumstance leads me to believe that by combining the Borda prism with a large diffraction grating spectrograph one might succeed in measuring the deviations and consequently the indices of air with a very great precision, much higher than any yet attained. Suppose then that one has at disposal such a spectrograph, with a plane grating ruled in the ordinary way on a

* *Loc. cit.*

† *Phil. Trans.*, 1840, Part II, and 1841, Part I.

surface 50 × 80 mm. with 14,400 lines to the English inch, and a collimator consisting of a refractor of 7 — 8 cm. aperture and 1.5 metres focal length. The objective of the camera should have the same aperture and a focal length of 2 metres.

The two tubes should be fixed with their optical axes making a constant angle of about 40°. The grating is placed on the horizontal circle of a theodolite in such a manner that its center line coincides with both the vertical axis of the circle and the point of intersection of the optical axes of the collimator and the camera. In this condition of things all the spectra may be brought into the field of view by turning the grating, and if the normal to the grating coincides with either of the optical axes the D line of the second order will be found in the field of view. Finally, the slit is provided with two small slides, by means of which the upper or lower half may be uncovered, so as to obtain on the photographic plate two juxtaposed images of the spectrum.

This having been arranged, and the slit being adjusted so that the rays issuing from the collimator are parallel, a Borda prism, filled with air at the same temperature and pressure as the surrounding air, is placed between the camera objective and the grating. Assuming that the glass faces of this prism are absolutely plane and everywhere of the same thickness, the interposition of the prism will influence neither the sharpness nor the position of a given spectral line. If now the air is exhausted from the prism the spectral line, while preserving its original sharpness, will be displaced toward one side or the other, according to the position of the prism, by an amount proportional to the refraction of air for the wave-length in question. By now turning the prism 180° the same displacement will take place, but in the opposite direction. The difference between the two positions of the line in these two positions of the prism will thus correspond to twice the deviation of the rays in passing through the evacuated prism. Supposing the angle of the prism to be 120°, and by the aid of the mean index of air $n = 1.000293$ the well known formula

$$n - 1 = \cot \frac{A}{2} \cdot \frac{\Delta}{2} \sin 1'' \quad (1)$$

gives for Δ the value

$$\Delta = 206''.$$

Thus the angular distance between the two positions of the line named above will be:

$$2\Delta = 412''.$$

XUM

To this angular distance corresponds in the focal plane of the camera of the spectrograph a linear distance

$$\delta = 4^{mm}$$

approximately. Such will therefore be the difference in position of two images of the line which will be obtained if in the two positions of the evacuated prism a photographic plate is exposed to the spectra formed by the upper and lower half of the slit of the collimator.

The numerous measures which I have had occasion to make on spectrograms of the same nature as those here in question have shown that the distance between two spectral lines may be measured on a good negative by means of a dividing engine with a probable error in general not greater than

$$\pm 0.002^{mm}.$$

Thus the distance between the two photographic images of the displaced line in the two positions of the prism may be determined with the same exactness, *i. e.* about $\frac{1}{200}$ of its value. Thus the probable error of the double displacement 2Δ, in so far as it depends on measures of the plate, will be only about

$$\pm \tfrac{1}{200} .400'' = \pm 0''.2$$

and consequently the probable error of the single displacement

$$d\Delta = 0''.1.$$

Differentiating equation (1) we find:

$$dn = \frac{1}{2} \cot \frac{A}{2} \, . \, d\Delta \, . \sin 1''$$

whence

$$dn = \pm 0.00000014.$$

Thus the probable error of the index of refraction of air for the observed line will not in general exceed *two units* in the seventh decimal place, *i. e.*, it will be three times smaller than the probable error which M. Mascart considers should be attributed to his final values.

I have assumed above that the slit should be placed exactly in the principal focus of the collimator for the special rays which form the line photographed. This would be necessary if one photographed in one case the undisplaced line, *i. e.* formedby the rays transmitted by the prism full of air, and in the other case the line displaced by the evacuated prism. In fact in these two cases the position of the focal plane of the camera would only be the same

in case the rays emanating from the grating were parallel. But this identity of position of the focal plane for the two lines, displaced and undisplaced, is evidently quite necessary in order to have the images of the same sharpness in the two negatives taken with the two halves of the slit. Consequently, if it were desired to measure the single displacement it would be necessary to adjust the slit each time so that the rays corresponding to the line employed should leave the objective of the collimator sensibly parallel. In the method described above, on the contrary, where the double displacement is measured, and in which the evacuated prism is fixed during both exposures of the photographic plate, this condition is no longer necessary, since in this case the sharpness of the images of the line photographed depends solely on the condition that the plate be exactly in the conjugate focal plane of the collimator slit.

From the equation

$$\delta = F \tan 2\Delta = 2\,F\Delta \,.\, \tan 1''$$

we obtain:

$$\frac{dF}{F} = \frac{-d\Delta}{\Delta}.$$

The condition

$$\frac{d\Delta}{\Delta} = \pm \frac{1}{2000}$$

therefore leads to the following:

$$\frac{dF}{F} = \mp \frac{1}{2000}$$

i. e., to reach the desired precision in the determination of the index of refraction the focal length of the camera objective must be known within $\frac{1}{2000}$ of its value, or in the present case to about 1^{mm}. But in employing Bessel's method for the determination of focal lengths with the modifications which I have described elsewhere* it is perfectly easy to satisfy this condition.

In addition to the quantities so far considered the absolute values of the required indices depend also on the temperature and pressure of the air which surrounds the prism. Denoting these quantities by t and H, and the index found directly and reduced to zero and 0.76^{m} by n and n_0, we have:

$$n_0 - 1 = (n-1)\,(1+\alpha t)\,.\,\frac{760}{H}.$$

Thus the correction which must be applied to $n-1$ to obtain its value at zero and 0.76^{m} will be:

* *Bulletin de l'Academie des Sciences de St. Petersbourg*, T. XXXII, p. 412.

$$C = (n-1)\left[(1+\alpha t)\frac{760}{H} - 1\right].$$

If the error resulting in the determination of t were 1°, the corresponding error of C would be

$$dC = 0.0000011$$

approximately, which indicates that in order to correspond to the desired precision of the determinations of the index the absolute value of the temperature must be known within about 0°.2. But this is a condition not difficult to satisfy, because during the two exposures of the plate, which require not more than one minute of time when the solar spectrum is used, the variations in the temperature of the air may be considered infinitely small.

As for the influence which an incorrect determination of the pressure of the air may have on the values of the indices of refraction, we readily find that this influence exceeds the prescribed limits only in case the error in H is greater than 0.5 mm., an error which may be easily avoided.

Finally, there remains one further source of error to be considered, which at first sight seems the most serious of all—the prismatic form of the glass plates forming the prism in the direction of the plane of dispersion. Admitting the existence of such a defect it is easily seen that the deviation Δ found in the manner described above is the algebraic sum of the deviations caused by the refraction of the air and by that of the plates. To determine the direction of this last deviation as well as to determine its numerical value we may proceed in the same manner described above, but leaving the prism filled with air at the same temperature and pressure as the surrounding air; for then the displacements of the spectral line would depend only on the refraction in the plates. But instead of this it would evidently be better if by a suitable arrangement of the experiment the error could be eliminated without determining it. This would be possible if after having finished a series of measures the faces of the prism were removed and once more cemented in such a manner that the side formerly turned toward the base of the prism were now directed toward the top and vice versa. Thus the direction of the refraction by the plates is reversed and if the deviations obtained in the first series were for example too great, a second series of measures would give values too small by the same amount, so that the mean of the two series would be freed from the error arising from the prismatic form of the plates.

The considerations presented above will suffice, I believe, to in-

XUM

dicate the exceptional precision of which the method in question is susceptible. In this connection two circumstances merit special attention as serving to remove the disturbances due to variations of temperature, the injurious effect of which one can never be sure of having avoided in the first form of the method. In fact, the prism having once been evacuated, all further action of the air-pump is without effect, and consequently all variations of temperature in the prism due to rarefaction and compression of the air will be eliminated. In the second place the time of about one minute necessary for the exposure of the photographic plate in the two positions of the prism is so short that the temperature of the surrounding air may be considered as sensibly constant during this interval. These advantages combined with the precision of spectrographic measures should suffice to assure to the values of the indices of refraction the exactness desired.

ASTRO-PHYSICAL NOTES.

All articles and correspondence relating to spectroscopy and other subjects properly included in ASTRO-PHYSICS, should be addressed to George E. Hale, Kenwood Observatory of the University of Chicago, Chicago, U. S. A. Authors of papers are requested to refer to last page for information in regard to illustrations, reprint copies, etc.

The Total Eclipse of April 16.—The following telegrams are from the Chicago *Tribune:*

VALPARAISO, April 16.—Observations of the total solar eclipse were taken today at Mina Aris, Harvard College Station. The weather was clear during all phases of the eclipse, with no passing cloud and no haze to mar the observation. Atmospheric conditions were all that could have been wished for, and the results will be satisfactory to the highest degree.

The corona, seen this morning, generally speaking, resembled the corona of 1871, as portrayed by Capt. Tupman, and complex, like that observed by Liais in 1857, which extended some 700,000 miles from the Sun. There were four streamers, two of which have a length exceeding the Sun's radius, or stretching out more than 435,000 miles. Several dark rifts were visible, extending directly outward from the Moon's limb to the utmost limit of the corona. Filaments were numerous about both the solar poles. Compared with the corona of Jan. 1, 1889, the corona just observed was more brilliant.

During the total eclipse today several flaming solar prominences attained great distinctness and brilliancy. Within the streamers no rapid movements were observed, but the impression of the scene was rather one of calm and tranquillity. The streamers were widely extended at the base, but not very long. The Moon appeared of almost inky darkness, with only enough illumination at the edge of the disk to make its rotundity conspicuous, while from behind the orb streamed out on all sides the radiant filaments, beams, and sheets of pearly light which formed an irregular star-like decoration with a black lunar globe in its center.

The inner corona was of dazzling brightness, but still more dazzling were the eruptive prominences which blazed through it, to use the words of Professor Young, "like carbuncles."

Generally the inner corona had quite a uniform altitude, forming a ring about four minutes of arc in width, but separated with more or less definiteness from the outer corona, which projected to a far greater distance and was much more irregular in shape. During totality the distinctness and brilliancy of several prominences were pronounced. One of these was 80,000 miles in height. The spectroscopic observations secured today are very promising. As the eclipse progressed the temperature of the air fell considerably below its normal. The lowest reading of the thermometer occurred several minutes after totality.

To be more explicit the outer corona was unequally extended and much larger than in 1879 or 1889, as was to have been expected during the present period of increasing solar activity. The eclipse observations showed conclusively that the Sun is now far from being quiescent, but is in a state of great disturbance. There was very distinct evidence of great spottedness and the presence of faculæ or bright torch-like streaks. The color of the corona was rather whitish than red and of a pale or pearly white hue. Upon review of all today's observations, it may be said that the corona was a confirmation of that of 1871 and that of 1857, as drawn by Liais. The photographs of the eclipse obtained at Mina Aris were numerous and satisfactory. PICKERING.

LONDON, April 16.—A dispatch from Bathurst, British Gambia, states that the British astronomers at the station on the Salum River in French Senegambia obtained fine observations of the eclipse of the Sun today.

VALPARAISO, April 19.—Professor Schaeberle sends the following account of his observations at Mina Bronces of the total solar eclipse:

Sunday was a pleasant day for viewing the eclipse. The corona was very brilliant and much extended in all directions. It was similar to the corona of the eclipse of 1883. Eight photographs 10×20 inches in size, were obtained with the heliograph, and ten photographs were secured with the six-foot telescope and the same number with the three-foot telescope. I also obtained six photographs with the one-foot telescope and seventeen camera photographs with the forty-foot glass. These photographs demonstrate enormous prominences and also show solar clouds nearly 100,000 miles in height floating through space. The polar rays and others were long, conspicuous and trumpet-shaped in outline. The outer corona was prominent. The preliminary results are a strong confirmation of my mechanical theory of the corona. I received valuable help from various persons, especially from King Gale.

VALPARAISO, April 18.—Professor Pickering has worked out some of his observations. Forty minutes after the first contact, about three minutes to 8 A. M., the light of the Sun was observed changing to a sickly yellow hue, and there was a perceptible, though faint, chill of the air. Twenty minutes later the corona was revealed around the Moon before the obscuration had become total. During the period of totality there were in the east and west dark tints like rain clouds lowering on the horizon. The colors were yellow and faint orange in general, the light resembling that of a faint dawn. But generally it was less dark than at past eclipses. Professor Obrecht took observations 900 feet higher than Professor Pickering. His data were scant, having been obtained only through the telescope. The temperature fell three degrees during totality. This was specially notable, the observer considers. The shadow-bands, or "fringes," were similar to hot air waves over an oven or ripples on a pond, having the velocity

XUM

of a fast walk. None of them were more than two inches in length. They were from one to three feet apart. They were seen before and after totality; before totality they were faint. The directions of movement were variable.

Professor Pickering says the results obtained with the differential spectroscope give twenty lines in the "reversing layer" of the solar atmosphere—the shallow stratum of gas lying just above the photosphere and known to contain the vapors of many elements commonly found on our globe. Twelve of these brilliant colored spectral lines were seen through the telescope. This is the first time that these lines have been successfully photographed. Professor Pickering also says that the entire spectroscopic and corona photographs taken Sunday are very satisfactory and promise to yield important data. Thirty-four seconds before the eclipse became total two rays of light issuing from the cusps were observed in violent motion. Before totality the corona showed a conical structure, with a network of fine filaments visible to the naked eye. There were four light streamers from the corona; the two upper were shorter than the lower ones. Seven prominences were observed. Douglass estimates that they attained a height of 80,000 miles. The expedition is satisfied with the photometric results. Photographs taken with a double image prism were secured.

LICK OBSERVATORY, MOUNT HAMILTON Cal., April 18.—A cipher telegram just received from Professor Schæberle in Chile brings information that the Lick Observatory expedition to observe the total solar eclipse has been successful in every respect. The mechanical theory of the solar corona, proposed by Professor Schæberle, has been verified. His telegram means that the picture made by him months ago was a true representation of the actual corona visible at the eclipse. This is an important verification of the very far-reaching theory. The expansion of the solar corona was first photographed at the California eclipse of Jan. 1, 1889, and was fully described in the Lick Observatory report of that eclipse. Its existence was doubted by various European astronomers, and the cloudy weather did not allow it to be plainly photographed at the eclipse of December, 1889. Now, however, Professor Schæberle telegraphs that it has been again successfully photographed at his station high up in the mountains. Fifty photographs have been secured by Professor Schæberle and his assistants, using three different telescopes. One of these instruments gives an image of the Sun four inches in diameter and the corona covers a plate 18x22 inches. The whole program was satisfactorily carried out. It is only proper to add that the expenses of the Lick Observatory expedition were generously provided for by a gift from Mrs. Hearst of California, to whom science owes a new debt.

The Absorption of Heat in the Solar Atmosphere is the title of a valuable paper read before the Royal Irish Academy on May 9, 1892, by Messrs. W. E. Wilson and A. A. Rambaut.

The experimental part of the research has been performed by Mr. Wilson, while Dr. Rambaut has supplied the carefully developed mathematical theory.

The method consists* in allowing a large image (80 cm. diam.) of the Sun, produced by a heliostat and concave mirror of 10 ft. focus, to drift across the face (2 mm. square) of a Boys' radio-micrometer. The deflection is recorded photographically, and seconds are automatically marked off on the curve by a pendulum.

The differential equations for the motion of the needle are given, as well as the methods of determining the constants of the equations.

*Wilson. This Journal, 1892, p. 49.

The process of making an observation is certainly very simple, though the subsequent reduction would become a trifle tedious. It would appear quite desirable to determine the constants frequently.

The intention is expressed of continuing these observations with considerable regularity throughout the eleven year period of solar activity. Such a work cannot fail to be of high value in the solution of some of the outstanding solar problems.

In the present paper numerical results are given for only two transits, on different days, so that it is difficult to compare the accuracy attained with that of other recent observations* upon this subject. From the differences between the values obtained along the eastern and western radii of the Sun the degree of accuracy would appear to be about the same as for those cited, and the absolute values do not greatly differ.

The disadvantages of the method are that it does not, in the arrangement described in this paper, permit of observations of the northern and southern hemisperes of the Sun, nor continuously of the true eastern and western extremities of the solar equator.

If the transit is not exactly central it cannot be utilized so as to be absolutely independent of previous central transits.

It would seem desirable to have a means of inverting the solar image so that the same should side alternately transit first and last.

It would also be advantageous for the observer to be able to follow the image as it drifts across, and thus assure himself whether casual irregularities were due to faculae visibly present on the disc. Perhaps this would be equally well attained by having a careful drawing or photograph (or best of all a "spectroheliogram") of the Sun made at the same time with the observed "thermal transit."

It is to be hoped that the instrument will also be directed to the study of the faculae and Sun-spots, where the photographic registry could perhaps be well dispensed with.

The paper concludes with a provisional—because dependent upon so few observational data—determination of the total absorption in the solar atmosphere. The modified Laplace formula is used, as well as his solution of the integral by a continued fraction.

The results do not materially differ from those already alluded to which depend upon many more observations. Further results of Mr. Wilson's work will be awaited with interest. F.

Examination of Photographic Lenses at Kew.—Major Darwin's report on the examination of photographic lenses at the Kew Observatory (Royal Society Proceedings, No. 318, p. 403,) contains much valuable information on the characteristic qualities of the various kinds of lenses, and may be recommended for perusal by the intelligent photographer in place of the frequently grossly incorrect "photographic optics" of the ordinary text book or "photographic instructor."

The object of the committee represented by Major Darwin was to devise a method for the rapid examination of photographic lenses sent to the observatory, with a view to issuing certificates on the payment of a small fee. Beside data obtained by inspection or by comparatively simple measurement, the statements of the certificate relate chiefly to the following qualities: curvature of the field, definition at the center with the largest stop, distortion, achromatism, astigmatism, and illumination of the field at different distances from the center. On account of

* Frost. This Journal, 1892, p. 720.

the necessity for keeping the expense as low as possible, all observations are made by visual methods which are fully explained in the report, and which seem to be unexceptionable.

Major Darwin considers that the most serious omission in the Kew examination is, that there is nothing to show the actinic transparency of the glass, as a slight yellow tinge in the lenses, which would not be noticed by the eye, might yet be sufficient to seriously affect the rapidity of the objective. It does not seem to us that a selective absorption insufficient to produce a yellow tinge that could be detected by the eye, would be appreciable in the photographically active part of the spectrum, which is practically included between λ 4000 and λ 4860.

On the History of the Bolometer.—Recent German experimenters have described a supposed new method of obtaining the very thin strips of platinum used in the bolometer, which is essentially as follows: a strip of platinum is placed on a thicker one of silver, the two are welded together, rolled out to any desired extent, and the silver backing is then dissolved away by nitric acid. Priority in such a detail as this is of no special consequence, but as a matter of fact, Professor Langley has employed this method since 1881, with the slight difference that copper is used as a backing instead of silver. No doubt the method was suggested by the early experiments of Wallaston in drawing fine platinum wire by an analagous process.

In connection with the bolometer may be mentioned some very small galvanometer mirrors recently made by Mr. Brashear for the Smithsonian Institution, as their construction involves some rather curious points in optical practice. The mirrors were required to have a diameter of 2½ millimetres, an accurately spherical figure with radius of one metre, and not to exceed in weight 3¼ milligrammes or one twentieth grain. The method first tried was to grind and properly figure one side of a little glass disc thick enough to retain its shape, and subsequently to reduce the thickness by grinding away the back until the desired weight was reached, but it was found that the mirror curled up at the edges on the concave side, so as to come out with a much shorter radius than that required. Experiment showed finally that it was necessary to start with a radius of 3.2 metres in order to have the radius come out one metre after grinding away the back of the glass, and as the mirrors were seldom considerate enough to preserve a spherical figure in the process of curling, a great many of them had to be made, in order to procure a few which came within the requirements. A large number were ground together, after the manner of spectacle lenses.

CURRENT CELESTIAL PHENOMENA.

PLANET NOTES FOR JUNE.

Mercury will be at superior conjunction at $10^h\ 30^m$ A. M. June 4, and will not be in position for naked-eye observations during this month. It may be possible to obtain telescopic views of the planet during the afternoon in the latter half of the month. Mercury will be in conjunction with Venus, 59′ north, June 14 at $8^h\ 39^m$ P. M. central time. There will also be a conjunction of Mercury and Mars June 27 at $10^h\ 19^m$ A. M., Mercury passing 25′ north of Mars.

Venus will be "evening star" during June, but will be too near the Sun for favorable observations. She will be in conjunction with the crescent Moon,

XUM

3° 52′ south, June 14 at 7h 21m P. M. The three celestial bodies, Mercury, Venus, and the Moon, will be quite near together on that evening.

Mars will be a little higher in the heavens than Venus, but will be overtaken by Mercury June 27. The distance of Mars from the Earth on June 15 will be about 231,000,000 miles. The apparent diameter of the planet will then be only 4″.

Jupiter has passed behind the Sun, and is now "morning planet," but is not yet in favorable position for observation.

Saturn will be visible during the first half of the night. During this month Saturn will make the turn of the loop in his apparent path through the constellation Virgo (see chart, p. 80), and will therefore appear to be nearly stationary among the stars. The planet will be recognized by its bright yellow color. The star just to the east of Saturn is the double star γ Virginis, a fine object for small telescopes. The Moon will pass by Saturn about a degree to the south on June 21, conjunction occurring at 10h 16m A. M. Saturn will be at quadrature, or just 90° east from the Sun, on June 27. The elevation of the Earth above the plane of the rings is now at about its minimum for this year, and during the remaining months the rings will gradually widen out so that more of their details may be seen. On June 9 the apparent major axis of the outer ring will be 40″.5, and the minor axis 4″.3.

Uranus will be found, about on the line between the constellations Virgo and Libra, a little over a degree east of the fifth-magnitude star λ Virginis. It is not visible to the naked eye, but may be easily recognized by its dull green disk with a telescope of moderate power. The Moon will pass Uranus, 1° 40′ to the north, June 23 at 10h,22m central time.

Neptune will be behind the Sun on the morning of June 1, and will not be in position for observation for several months.

MERCURY.

Date. 1893.	R. A. h m	Decl. ° ′	Rises. h m	Transits. h m	Sets. h m
June 5.....	4 58.5	+ 23 33	4 20 A. M.	12 00.9 P. M.	7 42 P. M.
15.....	6 32.0	+ 25 09	5 02 "	12 54.8 "	8 47 "
25.....	7 50.8	+ 22 48	5 54 "	1 34.1 "	9 14 "

VENUS.

June 5.....	5 35.8	+ 23 52	4 52 A. M.	12 38.2 P. M.	8 24 P. M.
15.....	6 29.6	+ 24 10	5 05 "	12 52.5 "	8 40 "
25.....	7 23.1	+ 23 17	5 24 "	1 06.6 "	8 49 "

MARS.

June 5.....	7 17.5	+ 23 28	6 22 A. M.	2 05.8 P. M.	9 50 P. M.
15.....	7 31.2	+ 23 00	6 13 "	1 53.9 "	9 35 "
25.....	7 58.2	+ 21 54	6 06 "	1 41.6 "	9 17 "

JUPITER.

June 5.....	2 58.2	+ 15 57	2 54 A. M.	10 01.9 A. M.	5 10 P. M.
15.....	3 07.0	+ 16 34	2 21 "	9 31.2 "	4 42 "
25.....	3 15.4	+ 17 07	1 48 "	9 00.4 "	4 13 "

SATURN.

June 5.....	12 26.1	− 0 03	1 25 P. M.	7 27.3 P. M.	1 30 A. M.
15.....	12 26.3	− 0 06	12 46 "	6 48.1 "	12 50 "
25.....	12 27.0	− 0 14	12 08 "	6 09.5 "	12 11 "

URANUS.

June 5.....	14 20.2	− 13 30	4 13 P. M.	9 21.1 P. M.	2 29 A. M.
15.....	14 19.1	− 13 25	3 32 "	8 40.7 "	1 49 "
25.....	14 18.4	− 13 22	2 52 "	8 00.6 "	1 09 "

NEPTUNE.

Date. 1893.	R. A. h m	Decl. ° ′	Rises. h m	Transits. h m	Sets. h m
June 5.....	4 39.2	+ 20 39	4 12 A. M.	11 41.8 A. M.	7 11 P. M.
15.....	4 40.8	+ 20 42	3 34 "	11 04.1 "	6 34 "
25.....	4 42.3	+ 20 45	2 56 "	10 26.3 "	5 56 "

THE SUN.

June 5.....	4 55.9	+ 22 38	4 18 A. M.	11 58.3 A. M.	7 39 P. M.
15.....	5 37.3	+ 23 21	4 16 "	12 00.3 P. M.	7 45 "
25.....	6 18.9	+ 23 23	4 18 "	12 02.5 "	7 47 "

Minima of Variable Sars of the Algol Type.

U CEPHEI.

R. A.....$0^h\ 52^m\ 32^s$
Decl.....+81° 17′
Period.....$2d\ 11^h\ 50^m$
1893.
June 2 3 A. M.
7 3 A. M.
12 3 A. M.
17 2 A. M.
22 2 A. M.
26 1 A. M.

S. CANCRI.

R. A.....$8^h\ 37^m\ 39^s$
Decl.....+ 19° 26′
Period.....$9d\ 11^h\ 38^m$
June 13 Midn.

δ LIBRÆ.

R. A.....$14^h\ 55^m\ 06^s$
Decl.....— 8° 05′
Period.....$2d\ 7^h\ 51^m$
June 1 10 P. M.
8 10 "

δ LYRÆ CONT.

June 15 9 P. M.
18 5 A. M.
22 9 P. M.
25 5 A. M.
29 8 P. M.

U CORONÆ.

R. A.....$15^h\ 13^m\ 43^s$
Decl.....+ 32° 03′
Period.....$2d\ 10^h\ 51^m$
June 18 4 A. M.
25 1 "

U OPHIUCHI.

R. A.....$17^h\ 10^m\ 56^s$
Decl.....+ 1° 20′
Period.....$0d\ 20^h\ 8^m$
June 4 3 A. M.
4 midn.
5 8 P. M.
9 4 A. M.
9 midn.

U OPHIUCHI CONT.

June 10 8 P. M.
14 5 A. M.
15 1 "
15 9 P. M.
20 2 A. M.
20 10 P. M.
25 3 A. M.
25 11 P. M.
26 7 "
30 3 A. M.

γ CYGNI

R. A.....$20^h\ 47^m\ 40^s$
Decl.....+ 34° 15′
Period.....$1d\ 11^h\ 57^m$
June 13 5 A. M.
16 5 "
19 5 "
22 5 "
25 5 "
28 5 "

Configura tion of Jupiter's Satellites at 8^h p. m. Central Time.

June		June		June	
1	4 1 ○ 2 3	11	♌ 3 ○ 2 4	21	4 ○ 2 1 3
2	4 2 3 ○ 1	12	4 3 ○ 1 4	22	4 1 ○ 2 3
3	4 3 2 ○ ●	13	2 1 ○ 3 4	23	♌ 4 2 ○ 1
4	4 3 1 ○ 2	14	○ 2 1 3 4	24	4 3 2 1 ○
5	4 2 ○ 1 ●	15	1 ○ 2 3 4	25	3 4 ○ 1 2
6	4 2 1 ○ 3	16	2 3 ○ 1 4	26	3 ○ 4 2 ●
7	4 ○ 2 1 3	17	3 2 1 ○ 4	27	2 1 ○ 3 4
8	4 1 ○ 2 3	18	3 4 ○ 1 2	28	○ 1 3 4 ●
9	2 3 ○ 1 ●	19	4 3 ○ 2 1	29	1 ○ 2 3 4
10	3 2 1 ○ 4	20	4 2 1 ○ 3	30	2 ○ 3 1 4

Phases and Aspects of the Moon.

	d	h	m	
Last Quarter.....	June 7	7	43	A. M.
Perigee.....	" 13	10	18	"
New Moon.....	" 13	11	51	P. M.
First Quarter.....	" 20	8	37	"
Apogee.....	" 26	7	42	A. M.
Full Moon.....	" 29	12	25	"

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Mr. Marth's Ephemerides of the Satellites of Saturn.

[From *Monthly Notices*, March 1892.].

In this table the times have been changed from Greenwich Mean Time to Central Standard Time. The abreviations *Rh.*, *Te.*, *Di.*, *En.*, and *Mi.*, stand for the names of the satellites Rhea, Tethys, Dione, Enceladus, and Mimas The letters *a*, *b*, *c*, *d*, and *e*, stand for conjunctions of the satellites in order as follows: With the preceding end of the outer ring; with preceding end of planet's equatorial diameter; with center of planet; with following end of planet's diameter; with following end of ring. The letters *n* and *s* signify that the satellite at the time of conjunction is north or south of the point designated by the preceding letter; *Sh.* means that the shadow of a satellite is near the central meridian of the planet; *Ecl. D.* and *Ecl. R.*, the disappearance and reappearance of a satellite at beginning and end of an eclipse. The letters e and w standing alone signify eastern and western elongations.

May 1893.			May 1893.			May 1893.			June 1893.		
15	2.1 am	Ti an	21	11.6 pm	Te bs	31	7.3 pm	Di ds	7	9.1 pm	Di en
	7.0 pm	Rh an	22	6.1	Rh inf		8.8	Rh bs		9.3	Te bs
	8.8	Mi es		6.8	En as		9.3	Mi es		11.0	Mi an
	9.5	Rh bn		8.0	Rh bs		10.6	Di bs		11.1	En es
	10.4	En as		8.2	Te dn		11.4	Rh as		11.3	Te as
	11.5	Rh cs		10.2	Te en	June			8	8.0	Te dn
16	12.8 am	Te es		10.5	Mi an	1	12.8 am	Di as		9.6	Mi an
	6.1 pm	Di bn		10.6	Rh as		7.1 pm	En an		9.9	En en
	7.5	Mi es		11.5	Di en		8.0	Mi es		10.0	Te en
	9.4	Di dn	23	7.8	Di as		10.0	Rh w		10.2	Di es
	11.4	Te an		8.9	Te bs	2	12.5 am	Te es	9	6.5	Di w
	11.6	Di en		9.1	Mi an		6.6 pm	Mi es		6.7	Te bs
17	6.1	Mi es		9.2	Rh w		8.6	Rh an		7.7	Rh inf
	10.1	Te es		9.4	En es		10.2	Di e		8.2	Mi an
	11.7	En en		10.9	Te as		11.1	Rh bn		8.7	Te as
	midn.	Mi as	24	7.5	Te es		11.2	Te an		9.6	Rh bs
18	12.1 am	Ti ds		7.7	Mi an	3	6.5	Di as	10	12.2 am	Rh as
	12.7	Di es		7.8	Rh an		8.5	En es		6.8 pm	Mi an
	1.2	Rh es		8.2	En en		9.8	Te es		7.3	Te en
	8.7 pm	Te an		8.9	Di an		11.1	Mi as		10.8	Rh w
	9.0	Di w	24	10.3	Rh bn		11.8	Te ds	11	6.0	Tr as
	10.6	Mi as		11.1	Di bn	4	7.2	En en		6.1	Di ds
	10.7	Te bn	25	12.3 am	Rh sup		7.7	Di an		9.4	Di ds
19	6.7	En es		6.2 pm	Te as		8.5	Te an		9.4	Rh an
	7.4	Te es		6.3	Mi an		9.7	Mi as		11.4	Mi en
	9.2	Mi as		10.7	En an		9.9	Di bn		11.6	Di as
	9.4	Te ds	26	9.5	En as		10.5	Te bn		midn.	Rh bn
20	12.3 am	Te bs	27	8.1	Di dn	5	6.1	Te es	12	6.2 pm	En an
	6.0 pm	Te an		9.5	Mi en		8.3	Mi as		10.0	Mi en
	6.4	Di es		10.3	Di en		9.1	Te ds	13	8.6	Mi en
	7.8	Mi as	28	8.1	Mi en		9.8	En an		9.0	Di e
	8.0	Te bn		10.8	En en	6	7.0	Mi as	14	7.2	Mi en
	8.6	Di ds		11.4	Di es		7.8	Te bn		7.6	En es
	10.9	Te dn	29	6.7	Mi en		8.6	En as	15	6.4	En en
	11.9	Di bs		7.7	Di w		10.7	Te dn		6.4	Di an
21	6.4	Mi as	30	10.7	Mi es	7	6.0	Rh en		8.6	Di bn
	6.7	Te ds	31	12.1 am	En as		6.4	Te ds			
	8.0	En an		6.9 pm	Rh inf		6.8	Di da			

Phenomena of Jupiter's Satellites.

1893.		h	m				1893.		h	m			
June	3	3	15	A. M.	I	Sh. In.	June	19	2	21	A. M.	I	Tr. In.
	4	3	19	"	I	Oc. Re.		20	1	49	"	I	Oc. Re.
	11	2	25	"	I	Ec. Dis.		20	3	02	"	II	Tr. In.
	12	2	36	"	I	Tr. Eg.		24	2	38	"	III	Tr. Eg.
	13	2	37	"	II	Tr. Eg.	July	1	2	54	"	III	Sh. Eg.
	13	3	03	"	III	Ec. Dis							

Occultations Visible at Washington.

				IMMERSION			EMERSION			
Date 1893.		Star's Name.	Magnitude.	Washington M. T.		Angle f'm N pt.	Washington M. T.		Angle f'm N pt.	Duration.
				h	m	°	h	m	°	h m
June	4	38 Capricorni	6.9	13	29	40	14	43	276	1 14
	22	*h* Virginis	5.8	7	01	138	8	27	303	1 26

New Minor Planets.—These are being discovered at such a rate that several alphabets are likely to be exhausted during this year if we continue to designate them by the present method:

	Mag.	Discoverer	Gr. M. T.	R. A.	Decl.	Daily Motion. R. A.	Decl.
			h m	h m s	° ′	s	′
1893 K	12.5	Charlois	Mar. 8 9 16	10 53 36	+ 6 13	−48	+7
L	9	"	9 8 34	10 08 48	− 0 43	−56	+3
M	13	"	10 10 57	11 41 28	+ 4 35	−52	+5
N	12	"	11 9 56	11 28 36	+14 06	−48	+7
O	12	"	11 9 56	11 31 56	+15 43	−52	+7
P	13	"	11 12 56	11 36 52	+11 49	−52	+3
Q	12	Wolf	16 12 00	11 11 24	+ 8 52	−40	+7
R	11	Charlois	17 10 52	12 19 16	+ 4 38	−56	+4
S	12	"	17 12 01	10 50 16	+16 50	−44	+2
T	12	"	19 10 26	11 52 52	+11 29	−52	+5
U	13	"	19 10 26	12 01 12	+14 20	−56	+8
V	13	"	21 10 11	12 08 52	− 2 09	−52	+6
W	12	"	21 10 11	12 20 36	− 7 25	−52	+2
X	12.5	Wolf	21 11 25	12 25 52	− 0 43	−40	+5

From the *Astronomical Journal*, No. 294, we learn that the asteroids of 1892 from O onward have received numbers as follows:

1892 O (Charlois Nov. 23) = No. 345
P (" " 25) = " 346
Q (" " 28) = " 347
R (" " 28) = " 348
S (" Dec. 8) not numbered.
T (" " 9) = No. 349
U (" " 14) = " 350
V (" " 16) = " 351

Mr. Charlois has assigned names, as below, to four of those discovered by him:

314 1891 Sept. 1, Rosalia.
316 1891 Sept. 8, Goberta.
317 1891 Sept. 11, Roxana.
349 1892 Dec. 9, Dembowski.

COMET NOTES.

No comets have been discovered so far this year, nor has the periodic comet of Finlay been yet picked up. We continue the search ephemeris for the latter below.

Ephemeris of Comet Finlay (1886 VII).

(From *Astr. Nachr.* 3154.)

Paris M. T.	App. R. A.	App. Decl.	log. Δ	$1 : r^2\Delta^2$
	h m s	° ′		
1893 May 16	23 31 25	− 6 09.6	0.1178	0.355
17	35 35	5 43.6		
18	39 46	5 17.3		
19	43 58	4 50.7		
20	48 12	4 23.9	0.1070	0.394
21	52 28	3 56.8		
22	23 56 45	3 39.4		
23	0 01 04	3 01.8		
24	05 24	2 33.9	0.0973	0.434
25	09 45	2 05.8		
26	14 08	1 37.5		
27	18 32	1 09.0		
28	22 57	0 40.4	0.0890	0.475
29	27 24	− 0 11.6		
30	31 52	+ 0 17.3		
31	36 22	1 46.4		
June 1	0 40 53	+ 1 15.6	0.0821	0.515

XUM

Our last sight of Holmes comet was had on the night of April 3, when the comet with the 16 inch telescope was exceedingly faint. It was perhaps 2′ in diameter with a very slight central condensation. It was impossible to obtain a micrometrical measure of the comet's position.

Mr. C. C. Hutchins sends us a sketch of Holmes' comet made with a 12-inch reflector, power 85, on the night of Nov. 20, which agrees closely with our own sketches about that time. He calls attention to the fact that the appearance of the comet was quite different from that indicated by the sketches of Mr. Denning, reproduced in the frontispiece of our last number. The tail of the comet, instead of narrowing and producing the pear-shape indicated by Mr. Denning, gradually widened, giving the usual paraboloidal form. The granular structure of the little bright tail back of the nucleus was not noticed, nor was there any dark space around it.

Elements of Comet 1893 I (Brooks *g*, 1892).—The following are the elements of Comet 1892 *g* as computed by Mr. Isham and myself from all observations available up to the present time:

$$\begin{aligned} T &= 1893, \text{ Jan. } 6.53308 \text{ Berlin M. T.} \\ \Omega &= 185^\circ\ 39'\ 17''.8 \\ i &= 143\ \ 51\ \ 48\ \ .4 \quad \Big\} 1893.0 \\ \omega &= \ \ 85\ \ 13\ \ 18\ \ .5 \\ \log q &= 0.0774075 \end{aligned}$$

and the equatorial co-ordinates for 1893

$$\begin{aligned} x &= [9.9992656]\, r \sin (350^\circ\ 38'\ 58''.5 + v) \\ y &= [9.7081212]\, r \sin (\ \ 75\ \ 01\ \ 31\ \ .3 + v) \\ z &= [9.9353820]\, r \sin (\ \ 82\ \ 36\ \ 48\ \ .3 + v) \end{aligned}$$

We did not think it worth while to prepare any ephemeris as it is scarcely possible that the comet will be visible. J. G. PORTER.

NEWS AND NOTES.

Inadvertantly we have sent a few numbers of this publication to some subscribers who have not renewed for this year, or ordered discontinuance. We are sorry to drop any names in this way, but we are obliged to do it, in view of the heavy expense incurred in publishing so much as we do each month. We must know what our support is to be for the year and plan accordingly. Notices are sent once more and the journal will be withheld until orders are received.

Popular Astronomy.—A great many letters have been received from different parts of the United States, Canada and Europe, in relation to suggestions in the April number of ASTRONOMY AND ASTRO-PHYSICS about a new publication devoted to "*Popular Astronomy*" to meet the wants of students, teachers, popular readers and amateurs. The interest manifested so far seems unexpectedly great. We now respectfully ask *every reader of this paragraph*, as a contribution to this worthy undertaking, that you send one or more names of those likely to be interested in such a monthly periodical to the publisher of this journal for the purpose of further correspondence. If such a publication is really needed, and there is sufficient support promised to make the undertaking successful financially, and useful in a high degree, it will be promptly undertaken. Otherwise it will not be tried. *Send us names.*

XUM

Screens to Protect Telescopes from Wind Tremors.—*To the Editor of Astronomy and Astro-Physics:* I have, naturally been quite interested in the discussion which has sprung up as to how to protect the great telescope at Mt. Hamilton from wind tremors, and venture to make a modest contribution to the subject in hand Some three years ago Mr. S. W. Burnham, then astronomer here, complained of such tremors, and I had a machinist put up two rods inside the dome, one on each side of the slit, and fit a series of loose sliding rings to each rod. We never went so far as to attach canvas to these rings, for Mr. Burnham finally decided that he did not wish it to be done on his account.

These rods and rings have been in place for several years, and any of the expedients suggested can be tried at any time in half an hour by anyone interested who knows of the existence of the rods, etc.

Professor Winnecke once advised me to have the slit of the great dome extended from horizon to horizon. I decided for a 90° (or so) slit, and the suggestions which we have heard prove that this decision was a correct one.

I am, dear sir, very respectfully,

Lick Observatory, 1893, April 20. EDWARD S. HOLDEN.

Missouri Botanical Garden—Banquet of the Trustees.—The Fourth Annual Banquet of the Trustees of the Missouri Botanical Garden will be held May 17, 1893. We notice the name of Professor Henry S. Pritchett, Director of the Observatory of Washington University, in the list of trustees. The following gentlemen have promised to be present and speak at the trustees' banquet:

President Wm. R. Harper, of the University of Chicago.
President R. H. Jesse, of the University of the State of Missouri.
President C. K. Adams, of the University of Wisconsin.
Professor T. C. Mendenhall, Superintendent of the U. S. Coast Survey.
Professor T. C. Chamberlin, of the University of Chicago.
Professor Geo. L. Goodale, of Harvard University.
Professor Asaph Hall, of Washington.
Professor C. O. Whitman, of the University of Chicago, and
His Excellency, Governor Wm. J. Stone.

The Large Nebula near ξ Persei (N. G. C. 1499).—In *Astronomische Nachrichten* No. 3157, Dr. F. Scheiner gives a drawing of this nebula from photographs of from 1 to 6 hours exposure. He used a Voightlander Euryscope lens of 4 inches aperture and focal distance 3⅓ to 1. He finds that with the 6-hour exposure the nebula is much more extensive than it has ever before been known to be, and approaches nearly the size of the Andromeda and Orion nebulæ. This nebula was discovered by Barnard with a 6-inch photographic lens. It extends, in Scheiner's drawing, from R. A. 3^h 56^m Decl. + 35° 15′ to R. A. 3^h 45^m Decl. 36° 40′ and is from 30′ to 50′ in width. There are suggestions of spiral curves but none so definite as those of the Andromeda nebula.

Astronomical Observations at the Royal Observatory of Prague for the years 1888, 1889, 1890 and 1891.—We have been favored with a copy of the Astronomical Observations made at the Royal Observatory of Prague during the years 1888 to 1891, by the director of the Observatory, Professor L. Weinek. It is a neat large quarto volume of 90 pages giving observations of moon culminations for the year 1888, observations of latitude by the Horrebow-Talcott method from 1889 to 1892, observations of comets Sawerthal 1888 I, Barnard 1889 I, and other phe-

nomena of interest during the time above mentioned. A very interesting part of the volume is devoted to special work in study of particular objects on the surface of the Moon. Some fifty pages of the text are given to the description of prominent objects chosen with full study of details. These subjects are illustrated by drawings and most beautifully executed plates by the heliographic process. The reproduction of one of the Moon's photographs taken at Lick Observatory by the aid of the 36-inch equatorial is an exquisite piece of work. The full page illustration of Professor Weinek's Meridian Circle room gives a good idea of his interesting work-shop.

Distances of the Stars by the Doppler's Principle.—An interesting paper appeared in the April number of the *American Journal of Science* in relation to stellar distances by the Doppler's principle. The author is Mr. G. W. Colles, Jr., and a brief review of his paper follows.

The first mention of the application of the Doppler's principle for finding stellar distances was by Fox Talbot in a paper read before the British Association in 1871, in which he showed how the principle might be applied in determining the distances of binary systems. His plan was to measure the relative velocities of each of the components of the binary when they were moving in the line of sight by the aid of the spectroscope. The shape of the orbit being known, the proportionate velocities of the stars at any point in it are known. Now since the *absolute* velocities at one point in the orbit are known, they can be deduced for a point where the stars are moving across the line of sight. Then find the angular velocity at this point, and divide the computed linear velocities by it, and the result will be the distance of the system.

In 1886 Professor A. A. Rambaut, Astronomer Royal of Ireland fully discussed Talbot's plan in papers published in the Proceedings of the Royal Irish Academy, and in the *Monthly Notices*, under the title of "On the Parallax of Double Stars." This principle could be extended to multiple systems, but for the lack of knowledge of the inclination of the plane of revolution in the line of sight. It can not be applied to find the distance of any single star, because the exact direction of that star's motion is not known. If any star should move rapidly enough, or instrumental measures should ever become accurate enough, so that an observer could detect the increase or decrease of its annual angular motion its distance might be determined approximately.

The author's purpose in this paper, however, is to suggest a much wider application of the Doppler's principle than those indicated above. It is to undertake the problem of the mean distance of all the stars, involving the theory of probability, in the hope, thereby, of gaining a more or less reliable idea of the extent of the cosmos.

His solution of this problem proceeds in this way: Suppose a *very* large number of stars to be distributed equally over the celestial sphere, with motions perfectly at random and represented by straight lines, and that the velocity in the line of sight and the proper angular motion of each star are accurately known, then the ratio of the mean velocity of the star across the line of sight to its mean velocity in the line of sight may be obtained. The analysis by which the mean distances of the stars are made a simple function of the mean angular velocity and mean velocity in the line of sight is worthy of a full statement which lack of space forbids. We can only say that the application of the formula derived gives as a mean distance of the brighter stars used, a distance of 9,596,000 astronomical units which equal 150.9 light-years. If the velocites of stars in the line of sight

XUM

as measured by Vogel are used, the result is less, 5,115,000 astronomical units or 80.5 light-years. Though at present this problem is beset by some difficulties almost insuperable, it will in time, offer advantages for determining stellar distances in a way not now even attempted.

The Mechanics of the Earth's Atmosphere.—Professor Cleveland Abbe has done excellent service to science in providing translations of some of the most important recent papers treating of the mechanics of the atmosphere. A pamphlet consisting of 324 pages, prepared by Professor Abbe for the Smithsonian miscellaneous collections contains:

1. *Hagen*, 1874. The measurements of the resistences experienced by plane plates when they are moved through the air in a direction normal to their planes.
2. *Helmholtz*, 1858. On the integrals of the hydrodynamic equations that represent vortex motions.
3. *Helmholtz*, 1868. On discontinuous motions in liquids.
4. *Helmholtz*, 1873. On a theorem relative to the movements that are geometrically similar, together with an application to the problem of steering balloons.
5. *Helmholtz*, 1888. On atmospheric motions, two papers, and a third on the theory of wind and waves.
7. *Helmholtz*, 1890. The energy of the billows and the wind.
8-13. *Overbeck*, 1882-1888. Four papers.

Then follow other papers by Hertz, Benzol, Rayleigh, Margules and Ferrel.

Catalogue of 3415 Southern Stars.—The American Academy of Arts and Sciences has recently published No. 1 of Vol. XII of its Memoirs, which is a catalogue of the magnitudes of southern stars from 0° to — 30° declination to the magnitude 7.0 inclusive. This work was done by Edwin F. Sawyer, well known observer of variable stars. It was begun in 1882, nearly finished in 1887, and completed in 1890. The whole number of observations is 13,654 on 3,415 stars, the average of observations to each star being 4. This catalogue may be regarded as an independent revision of Dr. B. A. Gould's *Uranometria Argentina*. The method of observation was by step-estimations, and each sequence comprised about ten stars, if enough desirable stars for comparison were near, if not, five or six; in some cases, twenty were used. The brightest in the region was first selected, then the next brighter, and so on down, other stars afterwards being inserted in their appropriate places. The various differences of brightness were estimated in steps. When all the stars in a given neighborhood had been so observed, a new sequence was begun and so on.

Nearly half of the whole work was done during the first year of observation, and the opera glass only was used. Afterward for the fainter sequences a field glass was employed. This would be necessary in observing sixth magnitudes and fainter ones. When a sequence was undertaken with any particular instrument that sequence would be completed with the same instrument. The observations were made with the instrument a little out of focus, so as to expand the light of the stars into a disc and comparisons were then made. This method was thought to be the most trustworthy, especially in the case of the colored stars. The observations were generally made during nights free from clouds and moonlight, and usually between the hours of 6 and 12 mean time.

The reductions of the sequences was done graphically on squared paper, the *Uranometria Argentina* magnitudes being used as ordinates and the observed differences of brightness expressed in steps as abscissas.

XUM

The care with which this piece of work seems to have been done and the ability of the observer put into it makes it a valuable catalogue. It will show amateurs what can be done with the opera glass and some patient practice in observing.

Paris Observatory in 1892.—From the annual report of the Director of the Paris Observatory for 1892 we take the following notes concerning the work of the Observatory during that year:

In the beginning of his first report the director, M. Tisserand, refers to the great loss which the institution sustained in the death of Admiral Mouchez and to the great work which the latter had initiated and in part successfully carried out, and announces his own determination so far as possible to carry these on to successful completion. At the suggestion of M. O. Struve, the presidency of the Permanent Committee on the Photographic Chart of the Sky has passed to the present director of the Paris Observatory. He has begun his endeavors to advance the project by increasing the personnel of the bureau for the measurements of the photographic plates at the Observatory and by having a new machine constructed for these measures.

The meridian circle work has been carried as heretofore, eleven observers taking part, and making a total of 16,686 observations. The great reflector has been devoted to spectroscopy, in charge of M. Deslandres, who has already determined the velocities in the line of sight of a number of stars. It is intended to determine the velocities of about 250 stars in this way. M. Deslandres has also succeeded in photographing solar prominences and faculæ and some new hydrogen lines.

The great Equatorial Coudé has been modified somewhat and is now ready for regular work. Other instruments have been used as heretofore in determining the places of comets and asteroids and in miscellaneous observations.

In the report of the bureau of measurements of the plates for the catalogue, are some interesting remarks on the magnitude of this work. Supposing each plate to contain 200 stars it requires 16 or 17 hours to measure one plate. Two persons with one machine could measure 130 plates per year. As each of the 18 observatories engaged in the work is expected to obtain from 1200 to 1400 plates, it would take the two persons ten years to measure the plates from one Observatory or 180 years to measure them all. Then the reductions of the measures would require further labor. To publish the catalogue would require 40 volumes of 1000 pages each with 50 stars to a page. And yet, some very eminent astronomers think that this ought to be done.

The Asteroid Collision Hypothesis—Answer to Mr. Holmes' Objections. The communication of Mr. Edwin Holmes to the "Comet Notes" of No. 114 of A AND A.-P. contains the statement of certain objections which he urges against the validity of the "asteroid collision" hypothesis of the origin of the celestial body which bears his name, that seem to require a reply from me. Considering these objections in the order of their importance, as I conceive it, the first is "that the mutual perturbations of the two asteroids in hypothetical collision, would operate to prevent such a catastrophe by shifting the bodies under consideration, so that they would pass by each other instead of colliding and that they would then form a "binary system" or a double asteroid. My answer to this is that the mutual perturbative action aforesaid might or might *not*, cause the effects stated. The question whether it would or would *not*, depends upon the masses of the asteroids, the elements of their orbits, and the directions of their motions, and it is *possible* for these

to have such values as to render the perturbation of the radius-vector extremely small, or even practically nothing. While it is true that two asteroids moving so that they would collide if undisturbed, can by perturbation be turned aside and pass each other, it is equally true that two such bodies, moving so that they would pass each other if there were no mutual perturbing force, could be brought into collision by their mutual attraction. Furthermore, in considering the motion of any celestial body, only that of a *material point*, that is the "centre of gravity" of the body, is regarded. Now, while it goes without saying that the collision of *material points* would be practically impossible, it should be noted that the asteroids, comparatively small though they be, are very far from being material points; their volumes are such that while it might be impossible for them to so collide that the impact would be "central," there is no reason why an "eccentric" impact could not occur. Moreover the masses of these bodies are so small that either of the two asteroids cannot materially change the velocity of the other unless they are very nearly in actual contact. As I have shown in No. 114 of A. AND A.-P., the velocity generated by the mass of an asteroid 74 miles in diameter (and this is a maximum value in the present case) would be only about 341 feet per second, at the very surface of such body. Now, unless the relative velocity of any two such asteroids were less than this, they could not form a "binary system" or a double asteroid; they would separate and move on around the Sun in slightly altered orbits. A relative velocity so small as 341 feet per second, is extremely improbable as between two asteroids; but admitting the existence of a "binary system" composed of two such bodies revolving freely around a common "centre of gravity," the perturbative action of the other bodies of the "solar system," particularly the larger ones, could so operate as to produce an "eccentric" impact between these asteroids, a possibility which suggested to me, when I first considered this subject, the hypothesis of a collision between the members of such a "binary system," but this hypothesis I rejected for certain good reasons, in favor of that of a direct collision. In view of the facts above set forth, I think that, in the face of the circumstantial evidence that has been produced, the weight of the objection stated above, is practically nil.

Another objection advanced by Mr. Holmes, is that such a collision should generate heat sufficient to render the matter self-luminous, or to reduce it to fluidity or even to the vaporous state in which there could be no nucleus, and that the spectroscope shows no such condition. This objection is quite fully answered in my communication to No. 114 of A. and A.-P., in which I have shown that the circumstances of the collision were such as to render the generation of a quantity of heat sufficient to produce the above described effects, or to give a characteristic spectrum, very improbable or even impossible. My investigation on this point has shown, to a practical certainty, that the effects, in this respect, were solely mechanical, consisting of the complete, or partial rupture of one or of both of the colliding bodies and the dispersion of the resulting particles, the work performed in overcoming the force of "cohesion," and in imparting velocity to the particles (to say nothing of the cooling by "radiation") using up the greater portion of the kinetic energy that would otherwise have been converted into heat and, therefore, leaving the body to shine only by reflected light, as the spectroscope indicates that it did.

A third objection advanced by the discoverer is "that the nebulous envelope of the body maintained a circular form (as it appeared to us) for three days after the date of discovery" which fact Mr. Holmes thinks incompatible with the idea of a "collision," although he says that subsequent developments appear to favor the "collision hypothesis." In answer to this I would say that, according to

that hypothesis the reasons why this nebulous envelope first appeared in circular form, are the following: Any asteroid can properly be regarded as a spherical body possessing some, and probably considerable, elasticity, and as one having on its surface more or less loosely associated and finely divided matter. To illustrate, we may regard each asteroid as a ball of iron, if indeed it be not, in reality, principally composed, like many meteoric bodies, of that metal or of similar substances. The first effect of the impact would be a concussion or an internal vibratory movement of the mass of the asteroid as a whole, and this would cause the projection of the loosely associated surface matter outward radially (this direction being in the line of least resistance) at a very high velocity owing to the very small masses of the surface particles, these being comparable to dust. Since the diameters of the two bodies in contact were so small that the contiguous bodies would form a mere point in comparison to the great diameter of the nebulous envelope, or to the distance to which the surface matter was projected, the latter must have practically assumed the form of a sphere and appeared to us as a circular nebulous disc. This action was the first to take place, and was entirely distinct from that which resulted subsequently in the elongation of the envelope, and the formation of the so called "tail," which were both mainly due to the movement of the particles resulting from the rupture, in their orbits around the Sun, resultants of the orbital motions of the original asteroids.

The futility of two of the three objections urged by Mr. Holmes against the asteroid collision hypothesis (the third having been already disposed of) can be even more forcibly demonstrated as follows: In the first place, in order that two asteroids each 60 miles in diameter, for instance, and moving so that they would collide centrally were there no mutual perturbative action between them, should by such action be caused to pass by each other, it is, evidently necessary that the center of gravity of each asteroid should by perturbation, be shifted 30 *miles*, the displacements being in opposite directions. Now, I have computed the maximum effect of this perturbative action upon each radius-vector in the case of two such asteroids having the masses given in No. 113 of A. and A.-P., and under the conditions existing in the present case, and I have found that such an effect would not exceed 66 *feet* in each case. Therefore the contention that mutual perturbation would prevent the collision of two such asteroids is futile.

Secondly, the reason for the *circular* form of the nebulous envelope during the first three days after discovery, can be illustrated as follows: An asteroid can, of course, be regarded as a body expansible by heat. We may also consider it as a sphere of iron 60 miles in diameter. Such a body out in the cold of space could be heated through about 1500° Fahr. before becoming self-luminous; that increase of temperature would cause expansion, the co-efficient of which, in the case of iron, would indicate an augmentation of about 1600 feet in the radius of the sphere. Therefore, if the increase of temperature and the consequent expansion took place in one second, loose matter on the surface of the sphere would be projected upward radially with a uniform velocity of 1600 feet per second, as by a shock or concussion, while an equal expansion in a shorter time would generate a greater velocity.

The loss of kinetic energy due to the impact, and the consequent heating and expansion of the colliding bodies must have occurred in a very brief interval, or almost instantaneously, and since it would require a velocity of only about 341 feet per second to project matter from the surface of such bodies so that it would never return, it is plain that the first effect of a collision would be to project loose surface matter radially outward at high and uniform velocity, and thus to form the spherical envelope which appeared to us as the *circular* nebulous disc.

XUM

Since all of Mr. Holmes' objections have thus been, I think, satisfactorily refuted, it follows that his conclusions that the "asteroid collision" hypothesis must be rejected," is unwarranted. On the contrary, since all the observed phenomena in this case are explicable by that hypothesis, and by none other that I know of, while the body discovered by Mr. Holmes has not exhibited any of the significant characteristics of a comet. I think that this "hypothesis" should be accepted if the recognized "criteria" determining the question of acceptance or of rejection, of any hypothesis, is not to be arbitrarily set aside in a most unscientific manner. In conclusion I would say that while I have, from the first recognized the great improbability of a collision between two asteroids considered as an abstract proposition, I have also been cognizant of the fact that even a *great improbability* is very far from being equivalent to an *absolute impossibility*. The matter is simply reduced to a question of evidence, and this I have endeavored to furnish in the two immediately preceding numbers of "ASTRONOMY AND ASTRO-PHYSICS." A collision at some definite point upon any ocean, between two vessels departing from widely separated ports and destined for points equally far apart, is a *very great improbability*, yet we know that such catastrophes have occurred, and not infrequently, and when we have the evidence of the wreckage even the *great improbability* counts for nothing.

Now, in the case under discussion, it is quite evident that astronomers have had before their eyes for several months past the *wreckage* resulting from the *collision* of two asteroids and which has been known as Holmes' comet.

SEVERINUS J. CORRIGAN.

ST. PAUL, Minnesota, April 8th, 1893.

Chicago Academy of Sciences.—Section of Mathematics and Astronomy.—The regular meeting of the Section was held at the Chicago Athenæum on Tuesday evening, March 7, 1893. Professor G. W. Hough presided.

Mr. A. C. Behr read a translation of Dr. Seeliger's paper on "General Problems in Celestial Mechanics. In the discussion Messrs. Burnham, Hough, Crew, See, Pike and Hale took part.

The second paper, on "Variations in the Calcium Spectrum from the Bunsen Flame to Sirius," was presented by Messrs. G. E. Hale and S. B. Barrett, and discussed by various members present. An abstract of this paper will be found on another page.

Adjourned. .

GEORGE E. HALE, Recorder.

The regular meeting of the Section was held at the Kenwood Observatory on Tuesday evening, April 4, 1893, Mr. R. W. Pike in the chair.

Professor G. E. Hale read a paper on "Present Limitations of Astronomical Photography," in which the difficulties due to coarseness of silver grain, lack of orthochromatism (a disadvantage only in the case of spectroscopic work, and in photography with a reflector), halation and enlargement of the image with prolonged exposure, and the impossibility of giving a correct exposure for objects of different degrees of brightness in the same field of view, were discussed and illustrated by lantern projections. It was shown that while in many directions photography has far surpassed visual observations, yet in others—such as the study of the Moon and planets, the minute details of Sun-spots, and the discovery and measurement of double-stars—it is still of little or no practical value.

In his remarks on the paper Mr. Burnham discussed Herr Weinek's alleged discovery of lunar rills, and showed that no amount of enlargement could bring out more detail on a photograph than is contained in the original—a point by no means generally understood. Referring to Dr. Robert's recent note on the absence of any indications of nebulosity in his photographs of Nova Aurigæ he remarked that the extremely faint nebulosity would require so prolonged an exposure that the image of the star would be greatly over-exposed, and the nebulosity would of necessity be completely hidden from view on the photographs.

Professor Hough discussed the sensitiveness of photographic plates and the employment of collodion plates in astronomical photography.

XUM

It was unanimously agreed that while it is not desirable to very greatly increase the sensitiveness of plates for most branches of astronomical photography, on account of the extreme difficulty of guarding against fog, every effort should be made to decrease the size of the silver grain, at the same time retaining at least the present degree of sensitiveness.

Adjourned. GEORGE E. HALE, Recorder.

New York Academy of Sciences.—Section of Astronomy and Physics.—Minutes of the meeting, 1893, April 3. The Section was called to order at 8:15 P. M., Professor Rees in the chair. The minutes of the previous meeting having been approved, a paper was read by Professor William Hallock entitled "Investigations of the Temperature of the Earth's crust." This paper gave an account of temperature measures made at Wheeling, W. Va., in a dry well 4,500 feet deep. These measures, when plotted, showed a small but distinct variation from uniformity in the rate of increase of the Earth's temperature. The results have been described in *Pro. Am. Assn.*, Vol. XL, p. 257; and *Am. Journ. Sci.*, 1892, March.

Mr. Tatlock then read a note on the place of λ Ursæ Minoris, calling attention to the bearing upon the subject of Dr. Elkin's recent heliometric triangulation of close polar stars. The matter was further discussed by Professor Safford and Mr. Jacoby. Professor Safford read a paper entitled, "The construction of a catalogue of standard polar stars." This paper dealt with the various peculiar difficulties attending the observation of close polar stars, as well as the complexity and great length of the resulting computations. The author referred to his own work in this direction, and stated that he hoped to publish before long a complete discussion of all existing observations of close polars.

HAROLD JACOBY, Sec'y of the Section.

Astronomical Society of the Pacific.—Annual meeting of the Astronomical Society of the Pacific was held in the lecture hall of the California Academy of Sciences, March 25, 1893. Vice President Molera occupied the chair. The following papers were presented:

Physical Observations of Jupiter's Satellites in Transit by John Tebbutt, of New South Wales; Solar Motion, by W. H. S. Monck, of Dublin; A Summary History of Astronomy in America from 1620 to 1893, by Edward S. Holden; Evolution of Double-Star Systems by T. J. J. See, of Chicago; Surface Markings of Mars (with lantern slides) by W. J. Hussey, of the Stanford University. Two new Planispheres by W. J. Hussey; Miscellaneous Observations of Nova Aurigæ, W. W. Campbell, of Mt. Hamilton; An Easy Method of Adjusting an Equatorial Telescope, by Roger Sprague, of Napa, Cal.; Astronomical Observations for 1892, by T. Köhl, of Odder, Denmark.

The report of the committee on the comet medal related to the calendar year, 1892. The comets for that year were:

Comet *a*; (unexpected) discovered by Lewis Swift, of Rochester, N. Y., March 6.

Comet *b*; (Winnecke's periodic) re-discovered by R. Spitaler, of Vienna, March 18.

Comet *c*; (unexpected) discovered by W. F. Denning, Bristol, England, March 18.

Comet *d*; (unexpected) discovered by W. R. Brooks, Geneva, N. Y., August 28.

Comet *e*; (unexpected), discovered by photography by E. E. Barnard, of Lick Observatory, October 12.

Comet *f*; (unexpected), discovered by Edwin Holmes, London, Eng., Nov. 6.

Comet *g*; (unexpected), discovered by W. R. Brooks, Geneva, N. Y., Nov. 19.

The comet medal has been duly transmitted to the discoverers of comets *a*, *c*, *d*, *e*, *f*, *g*, in accordance with the regulations. A copy of the comet-medal has been presented to the Royal Society of London, in the name of the Astronomical Society of the Pacific, in accordance with a resolution of the board of directors adopted Nov. 26, 1892.

The photograph of a meteor trail by John E. Lewis, of Ansonia, Conn., was exhibited to the members of the society. Mr. Molera gave an address and at its conclusion presented the society with twelve beautiful photographs, showing the buildings and instruments of the National Observatory of Tacubaya, Mexico. W. J. Hussey delivered a lecture on the planet Mars, illustrated by over one hundred slides of the most important drawings made of the markings of the planet from

the invention of the telescope to and including the last opposition. After other routine business the meeting adjourned.

The Astronomical and Physical Society of Toronto.—At the meeting of this society held March 21st, a long series of stellar planetary and solar observations was reported. The papers read included one by Mr. J. A. Copeland, on Holmes' Comet, and five upon Saturn. With a view to interesting members in that planet, a series of short papers was invited. More were offered than could be read. Those read included: "The Discovery of Saturn, his Rings and Moons," by Miss S. L. Taylor; "Saturn as a Habitable Globe," by Mr. G. G. Pleasey; "Phenomena Connected with the Disappearance of Saturn's Rings," by Dr. A. D. Watson; and "The Simple Mathematics of Saturn," by Mr. A. Harvey. The experiment was a success.

At the meeting of April 4th, a letter was read from Professor W. H. Pickering who expects to publish his report on Mars soon after his return to the United States which will take place in a few months, and who anticipates that the nomenclature of Mars will be one of the subjects dealt with by astronomers during the World's Fair. The paper of the evening was entitled "The Canals of Mars," and was contributed by Mr. S. E. Peal, F. R. A. S., of Sibsagar, Asam, India. Mr. Peal said that if the geological axiom of the permanent subsidence of ocean floors, so clearly seen on the Earth and Moon, applies to Mars, we can see at once that the completeness of the equatorial land girdle is due to the absence of tidal rupture by a large satellite and also an intelligible reason for the origin of the 'canals' as tideways open to the polar basins at each end, and that this peculiar arrangement was, there could scarcely be a doubt, due to the following causes: (a). That on Mars, the earlier phases of crust formation began at the poles, which, as time passed, became sea basins; that by the slow subsidence of the floors of these polar oceans, which would be the coldest and densest portions of the crust, the emergence of the equatorial land girdle would at last follow as a natural consequence; (b). That the comparative continuity of this latter would be assured by the absence of a large satellite causing tidal rupture, as in our case, the solar influence being, according to Professor Darwin, "inconsiderable." But though "inconsiderable," the solar influence would yet cause limited tides, a little before, during and after the equinoxes and tend to cause an "over-spill" from one basin into the other when one of the poles was turned toward the Sun. Such tide-water passing across the equatorial land girdle by the lowest levels would cause channels or "canals" which the "bore" would tend to straighten, especially if in alluvial strata. At the equinoxes the tides would, during the day time, be drawn up the canals from each polar basin on to the equatarial region by solar attraction, the return flow taking place at night. Thus, even with limited tides, the effectual circulation of water on Mars would probably much exceed that seen on our Earth and its heating by the solar rays to a large extent daily in the tropics would be greater than with us. The circulation of this heated water in each polar basin might well account for the smallness of the "polar caps," the net-work of canals across the equator acting as an efficient water heater, mitigating thereby the rigors of the arctic and antarctic climates. The occasional duplicity of the canals may possibly be due to the presence in them of a series of islands, like the sand "churs" of our Brahmapootra, a river which is very seldom, indeed, found to flow in one channel, and some of whose islands, like the "Majuli," or middle land, are 130 miles long by 10 to 20 broad. From an elevation of 20 to 30 miles, in fact, this river would undoubtedly present the appearance of a series of long loops.

NOTE.—Mr. Peal's paper will appear in full in *The Canadian Monthly* (Toronto) for May.

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tory, Greenwich; revised with additions and illustrations by David P. Todd, M. A., Ph. D., Professor of Astronomy and Director of the Observatory, Amherst College, Mass.

Total Eclipses of the Sun.—By Mrs. Mabel Loomis Todd.

ERRATUM.—Page 401, first line, for *eves* read *eaves*.

PUBLISHER'S NOTICES.

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James E. Keeler, Observatory, Allegheny, Pa.; Henry Crew, Northwestern University, Evanston, Ill.; Jos. S. Ames, Johns Hopkins University, Baltimore, Md.

All matter or correspondence relating to General Astronomy, remittances, subscriptions and advertising should be sent to Wm. W. Payne, Publisher and Proprietor of ASTRONOMY AND ASTRO-PHYSICS, Goodsell Observatory of Carleton College, Northfield, Minn.; and the Associate Editors for General Astronomy are: S. W. Burnham, Government Building, Chicago Ill.; E. E. Barnard, Lick Observatory, Mt. Hamilton, Cal., and H. C. Wilson, Goodsell Observatory, Northfield, Minn.

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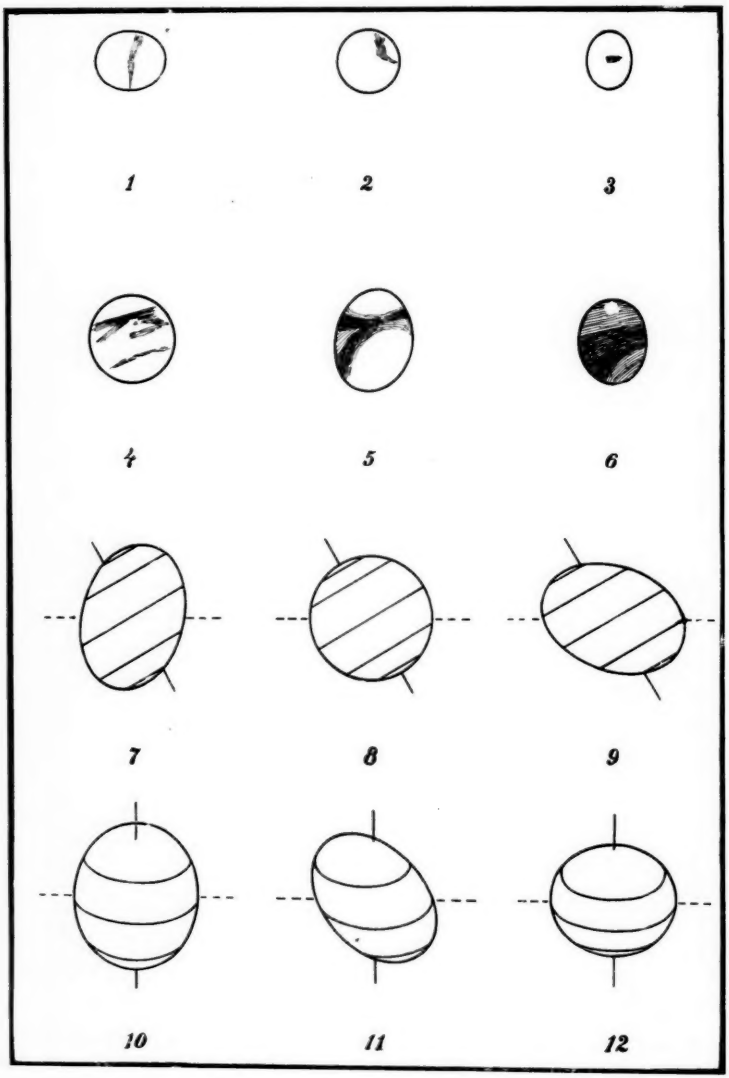
CONTENTS FOR APRIL.

General Astronomy: Holmes' Comet. Drawings by W. F. Denning, England. Plate XIX.....
Evolution of the Double Star Systems. Plates XX and XXI. T. J. J. See. 289
Relations between the Mean Motions of Jupiter, Saturn and Certain Minor Plantes. Daniel Kirkwood..... 302
Some Effects of a Collision Between Two Asteroids. (Illustrated). S. J. Corrigan..... 304
Dimensions of Small Planets. D. P. Todd..... 313
Neglected Field of Fundamental Astronomy. J. R. Eastman..... 315
Possibilities of the Telescope. Alvan G. Clark..... 319
Astro-Physics: A New Table of Standard Wave-Lengths, Henry A. Rowland..... 321
Note on the Spectrum of Sulphur. B. Hasselberg..... 347
Note on the Spectrum of Nova Aurigæ. William Huggins..... 349
Visual Observations of the Spectrum of β Lyræ. Plate XXII. James E. Keeler..... 350
Astro-Physical Notes.....362-366
Current Celestial Phenomena.....367-375
News and Notes.....376-383
Publisher's Notices..... 384

PLATE XXVI.



Jupiter's Outer Satellites.

XUM